BULLETIN

OF THE

PHILOSOPHICAL SOCIETY

OF

WASHINGTON.

VOL. IV.– VII

Containing the Minutes of the Society from the 185th Meeting, October 9, 1880, to the 202d Meeting, June 11, 1881.

PUBLISHED BY THE CO-OPERATION OF THE SMITHSONIAN INSTITUTION.

WASHINGTON:
1881.

1883, May 5 – 1886, April 3.
Gift of
The Society
(Vol. IV.–VI.)

JUDD & DETWEILER, PRINTERS,
WASHINGTON, D. C.

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CONSTITUTION

OF

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

ARTICLE I. The name of this Society shall be THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

ARTICLE II. The officers of the Society shall be a President, four Vice-Presidents, a Treasurer, and two Secretaries.

ARTICLE III. There shall be a General Committee, consisting of the officers of the Society and nine other members.

ARTICLE IV. The officers of the Society and the other members of the General Committee shall be elected annually by ballot; they shall hold office until their successors are elected, and shall have power to fill vacancies.

ARTICLE V. It shall be the duty of the General Committee to make rules for the government of the Society, and to transact all its business.

ARTICLE VI. This constitution shall not be amended except by a three-fourths vote of those present at an annual meeting for the election of officers, and after notice of the proposed change shall have been given in writing at a stated meeting of the Society at least four weeks previously.

STANDING RULES

FOR THE GOVERNMENT OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

JANUARY, 1881.

---

1. The Stated Meetings of the Society shall be held at 8 o'clock P. M. on every alternate Saturday; the place of meeting to be designated by the General Committee.

2. Notice of the time and place of meeting shall be sent to each member by one of the Secretaries.

When necessary, Special Meetings may be called by the President.

3. The Annual Meeting for the election of officers shall be the last stated meeting in the month of December.

The order of proceedings (which shall be announced by the Chair) shall be as follows:

First, the reading of the minutes of the last Annual Meeting.

Second, the presentation of the annual reports of the Secretaries, including the announcement of the names of members elected since the last annual meeting.

Third, the presentation of the annual report of the Treasurer.

Fourth, the announcement of the names of members who having complied with Section 12 of the Standing Rules, are entitled to vote on the election of officers.

Fifth, the election of President.

Sixth, the election of four Vice-Presidents.

Seventh, the election of Treasurer.

Eighth, the election of two Secretaries.

Ninth, the election of nine members of the General Committee.

Tenth, the consideration of Amendments to the Constitution of

the Society, if any such shall have been proposed in accordance with Article VI of the Constitution.

Eleventh, the reading of the rough minutes of the meeting.

4. Elections of officers are to be held as follows:

In each case nominations shall be made by means of an informal ballot, the result of which shall be announced by the Secretary; after which the first formal ballot shall be taken.

In the ballot for Vice-Presidents, Secretaries, and Members of the General Committee, each voter shall write on one ballot as many names as there are officers to be elected, viz., four on the first ballot for Vice-Presidents, two on the first for Secretaries, and nine on the first for Members of the General Committee; and on each subsequent ballot as many names as there are persons yet to be elected; and those persons who receive a majority of the votes cast shall be declared elected.

If in any case the informal ballot result in giving a majority for any one, it may be declared formal by a majority vote.

5. The Stated Meetings, with the exception of the annual meeting, shall be devoted to the consideration and discussion of scientific subjects.

The Stated Meeting next preceding the Annual Meeting shall be set apart for the delivery of the President's Annual Address.

6. Sections representing special branches of science may be formed by the General Committee upon the written recommendation of twenty members of the Society.

7. Persons interested in science, who are not residents of the District of Columbia, may be present at any meeting of the Society, except the annual meeting, upon invitation of a member.

8. Similar invitations to residents of the District of Columbia, not members of the Society, must be submitted through one of the Secretaries to the General Committee for approval.

9. Invitations to attend during three months the meetings of the Society and participate in the discussion of papers, may, by a vote of nine members of the General Committee, be issued to persons nominated by two members.

10. Communications intended for publication under the auspices of the Society shall be submitted in writing to the General Committee for approval.

11. New members may be proposed in writing by three members of the Society for election by the General Committee: but no person shall be admitted to the privileges of membership unless he signifies his acceptance thereof in writing within two months after notification of his election.

12. Each member shall pay annually to the Treasurer the sum of five dollars, and no member whose dues are unpaid shall vote at the annual meeting for the election of officers, or be entitled to a copy of the Bulletin.

In the absence of the Treasurer, the Secretary is authorized to receive the dues of members.

The names of those two years in arrears shall be dropped from the list of members.

Notice of resignation of membership shall be given in writing to the General Committee through the President or one of the Secretaries.

13. The fiscal year shall terminate with the Annual Meeting.

14. Members who are absent from the District of Columbia for more than twelve months may be excused from payment of the annual assessments, in which case their names shall be dropped from the list of members. They can, however, resume their membership by giving notice to the President of their wish to do so.

15. Any member not in arrears may, by the payment of one hundred dollars at any one time, become a life member, and be relieved from all further annual dues and other assessments.

All moneys received in payment of life membership shall be invested as portions of a permanent fund, which shall be directed solely to the furtherance of such special scientific work as may be ordered by the General Committee.

STANDING RULES

OF THE

GENERAL COMMITTEE OF THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

JANUARY, 1881.

---

1. The President, Vice-Presidents, and Secretaries of the Society shall hold like offices in the General Committee.

2. The President shall have power to call special meetings of the Committee, and to appoint Sub-Committees.

3. The Sub-Committees shall prepare business for the General Committee, and perform such other duties as may be entrusted to them.

4. There shall be two Standing Sub-Committees; one on Communications for the Stated Meetings of the Society, and another on Publications.

5. The General Committee shall meet at half-past seven o'clock on the evening of each Stated Meeting, and by adjournment at other times.

6. For all purposes except for the amendment of the Standing Rules of the Committee or of the Society, and the election of members, six members of the Committee shall constitute a quorum.

7. The names of proposed new members recommended in conformity with Section 11 of the Standing Rules of the Society, may be presented at any meeting of the General Committee, but shall lie over for at least four weeks before final action, and the concur-

1883, May 5 – 1886, April 3.

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Seventh, the election of Treasurer.

Eighth, the election of two Secretaries.

Ninth, the election of nine members of the General Committee.

Tenth, the consideration of Amendments to the Constitution of

rence of twelve members of the Committee shall be necessary to election.

The Secretary of the General Committee shall keep a chronological register of the elections and acceptances of members.

8. These Standing Rules, and those for the government of the Society, shall be modified only with the consent of a majority of the members of the General Committee.

RULES

FOR THE

PUBLICATION OF THE BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

JANUARY, 1881.

---

1. The President's annual address shall be published in full.

2. The annual reports of the Secretaries and of the Treasurer shall be published in full.

3. When directed by the General Committee, any communication may be published in full.

4. Abstracts of papers and remarks on the same will be published, when presented to the Secretary by the author in writing within two weeks of the evening of their delivery, and approved by the Committee on Publications. Brief abstracts prepared by one of the Secretaries and approved by the Committee on Publications may also be published.

5. Communications which have been published elsewhere, so as to be generally accessible, will appear in the Bulletin by title only, but with a reference to the place of publication, if made known in season to the Committee on Publications.

NOTE. *The attention of members to the above rules is specially requested.*

LIST OF MEMBERS

OF

THE PHILOSOPHICAL SOCIETY OF WASHINGTON,

Corrected to July 18th, 1881.

The names of the Founders of the Society, March 18, 1871, are printed in small capitals; for other members the dates of election are given.

§ indicates a life member by payment of 100 dollars.

* indicates absent from the District of Columbia, and excused from dues until announcing their return.

** indicates resigned.

? indicates dropped for non-payment of dues, or nothing known of him.

† indicates deceased.

N. B.—It is scarcely possible for the Treasurer to keep a correct record of those who are absent and excused from paying dues, unless members will keep him duly notified of their removals.

THOMAS ANTISELL.
Cleveland Abbe .... 1871, October 29.
Benjamin Alvord .... 1872, March 28.
Asa O. Aldis .... 1873, March 1.
Sylvanus Thayer Abert .... 1875, January 30.
Robert Stanton Avery .... 1879, October 11.

SPENCER FULLERTON BAIRD.
JOSEPH K. BARNES.
STEPHEN VINCENT BENÉT.
JOHN SHAW BILLINGS.
Orville Elias Babcock .... 1871, June 9.
Henry Hobart Bates .... 1871, November 4.
† Theodorus Bailey .... 1873, March 1.
Thomas W. Bartley .... 1873, March 29.
Samuel Clagett Busey .... 1874, January 17.

Emil Bessels .... 1875, January 16.
George Bancroft .... 1875, January 30.
* Lester A. Beardslee .... 1875, February 27.
* Rogers Birnie .... 1876, March 11.
Marcus Baker .... 1876, December 2.
Swan Moses Burnett .... 1879, March 29.
Alexander Graham Bell .... 1879, March 29.
William Birney .... 1879, March 29.
Horatio Chapin Burchard .... 1879, May 10.

HORACE CAPRON.
THOMAS LINCOLN CASEY.
† SALMON PORTLAND CHASE.
JOHN HUNTINGTON CRANE COFFIN.
† BENJAMIN FANEUIL CRAIG.
CHARLES HENRY CRANE.
Richard Dominicus Cutts .... 1871, April 29.
* Augustus L. Case .... 1872, November 16.
Robert Craig .... 1873, January 4.
Elliott Coues .... 1874, January 17.
Josiah Curtis .... 1874, March 28.
John White Chickering .... 1874, April 11.
* Frank Wigglesworth Clarke .... 1874, April 11.
Edward Clark .... 1877, February 24.
Frederick Collins .... 1879, October 21.
Thomas Craig .... 1879, November 22.
John Henry Comstock .... 1880, February 14.
Alexander Smythe Christie .... 1880, December 4.

WILLIAM HEALEY DALL.
† ALEXANDER B. DYER.
Clarence Edward Dutton .... 1872, January 27.
† Richard Crain Dean .... 1872, April 23.
Henry Harrison Chase Dunwoody .... 1873, December 20.
† Charles Henry Davis .... 1874, January 17.
† Frederic William Dorr .... 1874, January 17.
Myrick Hascall Doolittle .... 1876, February 12.
** George Dewey .... 1879, February 15.
Charles Henry Davis .... 1880, June 19.
Theodore Lewis DeLand .... 1880, December 18.
† AMOS BEEBE EATON.
EZEKIEL BROWN ELLIOTT.
** GEORGE H. ELLIOT.
John Robie Eastman .... 1871, May 27.
* Stewart Eldredge .... 1871, June 9.
Fredric Miller Endlich .... 1873, March 1.
? Charles Ewing .... 1874, January 17.

* Hugh Ewing ---------- 1874, January 17.
John Eaton ---------- 1874, May 8.

* ELISHA FOOTE.
William Ferrel ---------- 1872, November 16.
Edgar Frisby ---------- 1872, November 16.
† John Gray Foster ---------- 1873, January 18.
Edward T. Fristoe ---------- 1873, March 29.
Robert Fletcher ---------- 1873, April 10.
Edward Jessop Farquhar ---------- 1876, February 12.

THEODORE NICHOLAS GILL.
* BENJAMIN FRANKLIN GREEN.
Henry Goodfellow ---------- 1871, November 4.
Grove Karl Gilbert ---------- 1873, June 7.
Leonard Dunnell Gale ---------- 1874, January 17.
* James Terry Gardner ---------- 1874, January 17.
George Brown Goode ---------- 1874, January 31.
Henry Gannett ---------- 1874, April 11.
* Edward Oziel Graves ---------- 1874, April 11.
Edward Miner Gallaudet ---------- 1875, February 27.
Francis Vinton Greene ---------- 1875, April 10.
Francis Mathews Green ---------- 1875, November 9.
Edward Goodfellow ---------- 1875, December 18.
Alexander Young P. Garnett ---------- 1878, March 16.
* Walter Hayden Graves ---------- 1878, May 25.
* Francis Mackall Gunnell ---------- 1879, February 1.
Bernard Richardson Green ---------- 1879, February 15.
William Whiting Godding ---------- 1879, March 29.
James Howard Gore ---------- 1880, March 14.
* Adolphus W. Greely ---------- 1880, June 19.
Albert Leary Gihon ---------- 1880, December 18.

ASAPH HALL.
WILLIAM HARKNESS.
FERDINAND VANDEVEER HAYDEN.
† JOSEPH HENRY.
JULIUS ERASMUS HILGARD.
ANDREW ATKINSON HUMPHREYS.
Henry W. Howgate ---------- 1873, January 18.
* Edward Singleton Holden ---------- 1873, June 21.
† Isaiah Hanscom ---------- 1873, December 20.
* Edwin Eugene Howell ---------- 1874, January 31.
Henry Wetherbee Henshaw ---------- 1874, April 11.
David Lowe Huntingdon ---------- 1877, December 21.
George William Hill ---------- 1879, February 1.

*Peter Conover Hains .... 1879, February 15.
*Franklin Benjamin Hough .... 1879, March 29.
William Henry Holmes .... 1879, March 29.
Ferdinand H. Hassler .... 1880, May 8.
William B. Hazen .... 1881.

THORNTON ALEXANDER JENKINS.
William Waring Johnston .... 1873, June 21.
*Henry Arundel Lambe Jackson .... 1875, January 30.
William Nicolson Jeffers .... 1877, February 24.
Arnold Burgess Johnson .... 1878, January 19.
Joseph Taber Johnson .... 1879, March 29.
Owen James .... 1880, January 3.

*Reuel Keith .... 1871, October 29.
John Jay Knox .... 1874, May 8.
Albert Freeman Africanus King .... 1875, January 16.
†Ferdinand Kampf .... 1875, December 18.
**Clarence King .... 1879, May 10.
Jerome H. Kidder .... 1880, May 8.
Charles Evans Kilbourne .... 1880, June 19.

†JONATHAN HOMER LANE.
Nathan Smith Lincoln .... 1871, May 27.
**Henry H. Lockwood .... 1871, October 29.
**Stephen C. Lyford .... 1873, January 18.
William Lee .... 1874, January 17.
*Edward Phelps Lull .... 1875, December 4.
Eben Jenks Loomis .... 1880, February 14.

†FIELDING BRADFORD MEEK.
MONTGOMERY CUNNINGHAM MEIGS.
†ALBERT J. MYER.
William Myers .... 1871, June 23.
†Oscar A. Mack .... 1872, January 27.
William Manuel Mew .... 1873, December 20.
†Archibald Robertson Marvine .... 1874, January 31.
†James William Milner .... 1874, January 31.
Garrick Mallery .... 1875, January 30.
Otis Tufton Mason .... 1875, January 30.
William McMurtrie .... 1876, February 26.
Aniceto Gabriel Menocal .... 1877, February 24.
Martin Ferdinand Morris .... 1877, February 24.
*Montgomery Meigs .... 1877, March 24.
*Joseph Badger Marvin .... 1878, May 25.
Fredrick Bauders McGuire .... 1879, February 15.
? Clay Macauley .... 1880, January 3.

SIMON NEWCOMB.
WALTER LAMB NICHOLSON.
*Charles Henry Nichols .... 1872, May 4.
Charles Nordhoff .... 1879, May 10.

†GEORGE ALEXANDER OTIS.
John Walter Osborne .... 1878, December 7.

JOHN GRUBB PARKE.
PETER PARKER.
*TITIAN RAMSAY PEALE.
†BENJAMIN PIERCE.
Charles Christopher Parry .... 1871, May 18.
** Carlisle P. Patterson .... 1871, November 17.
* Charles Sanders Pierce .... 1878, March 1.
Orlando Metcalf Poe .... 1878, October 4.
John Wesley Powell .... 1874, January 17.
** David Dixon Porter .... 1874, April 11.
* Albert Charles Peale .... 1874, April 11.
Robert Lawrence Packard .... 1875, February 27.
Henry Martyn Paul .... 1877, May 19.
* Henry Smith Pritchett .... 1879, March 29.
Daniel Webster Prentiss .... 1880, January 3.

* Christopher Raymond Perry Rodgers .... 1872, March 9.
* Joseph Addison Rogers .... 1872, March 9.
John Rodgers .... 1872, November 16.
* Henry Reed Rathbone .... 1874, January 17.
* Robert Ridgway .... 1874, January 31.
† John Campbell Riley .... 1877, May 19.
Charles Valentine Riley .... 1878, November 9.
William Francis McKnight Ritter .... 1879, October 21.

BENJAMIN FRANKLIN SANDS.
† GEORGE CHRISTIAN SCHAEFFER.
CHARLES ANTHONY SCHOTT.
WILLIAM TUCUMSEH SHERMAN.
James Hamilton Saville .... 1871, April 29.
Ainsworth Rand Spofford .... 1872, January 27.
? Frederic Adolphus Sawyer .... 1873, October 4.
John Sherman .... 1874, January 17.
* John Stearns .... 1874, March 28.
* Ormond Stone .... 1874, March 28.
? Aaron Nicholas Skinner .... 1875, February 27.
Samuel Shellabarger .... 1875, April 10.
David Smith .... 1876, December 2.
Edwin Smith .... 1880, October 23.

* Montgomery Sicard	1877,	February 24.
Henry Robinson Searle	1877,	December 21.
Charles Dwight Sigsbee	1879,	March 1.
John Patten Story	1880,	June 19.
WILLIAM BOWER TAYLOR.		
William Calvin Tilden	1871,	April 29.
? George Taylor	1873,	March 1.
Joseph Meredith Toner	1873,	June 7.
Almon Harris Thompson	1875,	April 10.
William J. Twining	1878,	November 23.
David P. Todd	1878,	November 23.
** Jacob Kendrick Upton	1878,	February 2.
Winslow Upton	1880,	December 4.
George Vasey	1875,	June 5.
* JUNIUS B. WHEELER.		
JOSEPH JANVIER WOODWARD.		
William Maxwell Wood	1871,	December 2.
Francis Amasa Walker	1872,	January 27.
James Clarke Welling	1872,	November 16.
James Ormond Wilson	1873,	March 1.
* George M. Wheeler	1873,	June 7.
* John Maynard Woodworth	1874,	January 31.
Allen D. Wilson	1874,	April 11.
? Charles Warren	1874,	May 8.
* Joseph Wood	1875,	January 16.
* Christopher Columbus Wolcott	1875,	February 27.
Lester Frank Ward	1876,	November 18.
Charles Abiathar White	1876,	December 16.
Zebulon L. White	1880,	June 19.
William Crawford Winlock	1880,	December 4.
† Mordecai Yarnall	1871,	April 29.
Henry Crissey Yarrow	1874,	January 31.
Anton Zumbrock	1875,	January 30.

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

185TH MEETING. OCTOBER 9, 1880.

The President in the Chair.

The minutes of the last meeting were read and adopted.

The President notified the meeting of the decease of Prof. PEIRCE' whereupon

Mr. ELLIOTT moved the appointment of a committee of three, to be appointed by the Chair, to draft resolutions in accordance with the notice just given and submit the same at the next meeting.

The Chair appointed as Committee: J. E. HILGARD, J. H. C. COFFIN, and WM. FERRELL.

The treasurer notified the meeting that Vol. 3 of the Bulletin had been published, and that a copy would be forwarded to all members not in arrears.

Mr. C. ABBE communicated the first part of a paper on the Aurora Borealis, referring to studies made by him on the appearance of the aurora of April 4, 1874. He spoke of the difficulty which beset the consideration of the explanation of the appearance of the aurora, and especially of obtaining the altitude of the arch. The present modes of measuring the height yield only negative results, as shown by the experiments of Bravais and Martin, using the trigonometrical method. The second mode employs the varying amount of dip at separate localities, using it according to Galles' method, which assumes the dip of the needle to be of the same amount in the upper regions of the air as at the earth's surface, which has not been proved. Mr. Abbe also referred to Gauss' formula for calculating the direction and intensity of magnetism for all localities, and the defects in Galles' method of calculating the heights of auroras, and concluded that we should look with doubt upon all results obtained.

Mr. ABBE then alluded to a third method which has been used by Prof. Newton: this method is based on the assumption that the Aurora describes an arc running round the earth in a circle parallel to the region of greatest frequency of the aurora; this method involves too many assumptions to justify its adoption. It seems impossible to obtain harmonious results from observations at one locality compared with another; nor can the results be made to harmonize with the three methods.

Mr. ELLIOTT alluded to a generally accepted belief that auroras exist at variable heights in the atmosphere, and synchronous with its existence disturbance of the magnetic needle occurs and great electric disturbance, shown by the irregular working of telegraphic apparatus. In the high regions of the air the currents encounter much less resistance than at the earth level.

Mr. OSBORNE made remarks on observations made by him on auroras at Melbourne, and on the appearances of the magnetic light in the southern hemisphere.

Mr. POWELL considered that auroras could occasionally appear in the lower strata of the atmosphere, and referred to an observation of his own in which the arch was placed between the observer and a mountain.

Mr. FARQUHAR called attention to the frequent accounts given of the occurence of the aurora at low levels in high latitudes (as in Norway;) and as regards the direction of the flashing of the rays as proceeding from below upwards or *vice versa*, this might be an error of observation, similar to observations on the direction of currents or direction of electric light or of magnetism.

The President remarked in closing the discussion that more careful and systematic observations were necessary to determine the height and position of the auroral streamers, and to substantiate the conclusion that the same streamers could not be seen by observers a few miles apart. He cited the general fact of auroras being seen in the north and not in the south over wide stretches of latitude; as one which seems to him difficult to explain on any theory that the aurora was a local phenomenon.

The meeting then adjourned.

186TH MEETING. OCTOBER 23, 1880.

The President in the Chair.

The minutes of last meeting were read and adopted.

The President notified the meeting of the decease of General A. J. MYER, one of the members of the Society.

Dr. TONER moved the appointment of a committee to draft resolutions suitable to the occasion.

Committee appointed: Messrs. J. C. WELLING, CLEVELAND ABBE, GARRICK MALLERY.

The committee appointed at the last meeting of the Society, to report a resolution commemorative of the decease of Prof. PEIRCE, reported as follows:

Resovled, That the Philosophical Society of Washington put on record their appreciation of the eminent services to science rendered by the late Prof. Benjamin Peirce, of Harvard University, some time since Superintendent of the United States Coast Survey, and during that time a member of this Society. His introduction of the new modes of condensed mathematical thought into celestial mechanics, and his development of new algebraic methods to their uttermost limit, will ever mark him as one of the most powerful mathematicians of our age.

Mr. ALVORD said he had a warm sympathy with this just and appropriate tribute to the memory of Benjamin Peirce. Though he could say much in admiration of his genius and of his works, he would now only make an allusion to a mathematical discussion in which Prof. Peirce referred to his friend Agassiz, for whom he always expressed a warm regard.

In the spring of 1865 Prof. Peirce invited the speaker to attend the meeting, at Northampton, in August of that year, of the National Academy of Science, at which he expected to read a paper. On reaching the room was found arranged around the walls about a dozen large drawings to illustrate the "*Path of the Sling*," which was his topic. He had obtained an equation of this path. The curve exhibiting this path was very simple in his first drawings and very complicated in the last, according to the changes made in the constants entering into the equation, but the law on the equation of the curve remained the same. The last drawings disclosed highly complex and involved curves not unlike the epicycloids.

Prof. PEIRCE said that these drawings had greatly interested Prof. Agassiz, then absent in his voyage around Cape Horn. It was a striking example of the great varieties and possibilities in nature, buried in the same law. These curves, however apparently different, were traced by the use of the same identical equation, and between the examples exhibited by Prof. Peirce of course myriads of intermediate curves existed. It is obvious that the attraction of all this to Agassiz was the anolagy to organisms in botany and in zoology where groups and species obey some common generalization.

A son of Prof. Peirce has stated that this discussion was never printed, and it is feared that a large share of his brilliant original conception will never be published.

Mr. ELLIOTT referred in warm terms to the genial disposition of Prof. Pierce, and to the encouragement always given by him to young investigators, a characteristic by which he was marked.

Mr. ELLIOTT mentioned that he was the fortunate possessor of a presentation copy of the "Linear Associative Algebra" referred to by Prof. Hilgard, a work which could not fail to impress the investigator with respect and admiration for the great genius of the author.

Prof. HILGARD said he would supplement his first characterization of the ideal algebra, and would call that work the exhaustive treatment of a given mode of investigation, a method of research carried to its uttermost limit and completely exhausted.

Mr. ALVORD stated that Prof. Peirce undoubtedly did a good deal to further the cause of astronomical science by obtaining appropriations to test the value of heights on the Union Pacific Railroad for astronomical observations. In August, 1868, at Chicago, the American Association for the Advancement of Science recommended the establishment of an observatory in that region. Prof. Peirce, as Superintendent of the Coast and Geodetic Survey, had observations made at Sherman Station by Prof. C. A. Young, and on the Sierra Nevada by Prof. Davidson. All this paved the way for the endowment and establishment of the Lick Observatory. These experiments led to the conclusion that the atmosphere of California was most favorable to such observations. The more recent tentative observations of Mr. Burnham at Mount Hamilton confirm these views, and give promise of great success at the Lick Observatory.

Prof. ABBE said that while the scientific and public works of Prof. Peirce would always be spoken of with admiration, his social characteristics were equally interesting. Prof. Abbe could never forget the first time he shook hands with the venerable mathematician in 1860, when he felt that there was a bond of union and sympathy between them. Almost the first words he ever heard him utter gave a glimpse of the man himself. He had heard Prof. Peirce say that the true poet—he who writes the most elevated poetry—is the pure mathematician.

Remarks by Mr. EDWARD GOODFELLOW.

It was my privilege, more than a quarter of a century ago to be ordered to duty under Prof. Peirce's direction, to aid him in certain investigations he was making in behalf of the Coast Survey, with the object of ascertaining the most probable value to be assigned to observations of moon culminations in the determination of differences of longitude.

He was then in the prime of life and upon the threshold of that great fame which his works brought to him but a few years later. He impressed me as a man of thorough kindliness of heart. I came to Cambridge an entire stranger; he interested himself personally in obtaining for me home-like lodgings, and not unfrequently would come to my room to explain in detail, or to write out at length, formulæ which in his own very concise forms had been to me an entire puzzle.

Among the Harvard students he was very popular; his text-books though were less liked than himself. It was a common saying among the collegians, that Prof. Peirce took for granted, in his books, that every one had as clear an insight into mathematics as he himself had.

I was on duty at West Hills, one of the Coast Survey stations on Long Island, in 1865, when Prof. Peirce came to see Mr. Bache, then just returned from Europe, but not with improved health.

Two years later, the death of Prof. Bache created a great vacancy. At that time the character and qualifications of the man who should succeed him in that high office were thoroughly understood. A recognized pre-eminence among scientific men, an ability to form an independent judgment respecting the problems of geodesy involved in the work—these were essentials. It is enough to say of Prof. Peirce that his appointment amply fulfilled these requirements. Foremost among the geometers of his own land, and regarded as in the front rank of foreign mathematicians, Prof. Peirce, during the first years of his superintendency, developed an administrative ability, which, in the methods of its exercise, won for him the friendly regard and respect of both the older and younger officers of the survey. Recognizing, with a fine tact and courtesy, the conditions entailed upon officers engaged in field work—much physical hardship, small pay, and slow promotion—he established a system of gradual increase of pay at certain intervals, and according to merit.

With Government officials, members of Congress, and all whom it was necessary to consult in obtaining appropriations for the survey, Prof. Peirce was never at fault; he knew how to use the legitimate methods of success; and he will long be remembered, not only as a great mathematician, but as the able director of an important national work.

President NEWCOMB said, as one who had known Prof. Peirce only a little less than a quarter of a century, it might not be inappropriate for him to say a few words, although much that he would have said had been anticipated by those who had already addressed the Society.

One of the most interesting points in Prof. Peirce's character was the fact that he was anything but a mathematician, as conventionally understood—cold, unsympathizing, living in an atmosphere above the rest of the world. Prof. Newcomb had never known any one who had a better heart.

Several members had spoken of the encouragement given by Prof. Peirce to those who first entered upon their life career. The speaker's first interview with that distinguished mathematician had been indelibly impressed upon his mind. What struck him most forcibly about Prof. Peirce at that time was the perfectly unsophisticated way in which he put one at ease, and the total freedom from anything like dignity or pretentiousness which one might suppose would be seen in so great a man. An interesting trait in Prof. Peirce's intellectual character was his disposition to look at the philosophical side of things. Altogether, his mathematical works were as much treatises on formal logic as they were on formal mathematics. The paper on multiple algebra, referred to by Prof. Hilgard, had very much of that character.

Prof. Peirce's method of judging men was peculiar. Among his students he recognized only two classes—those who knew and those who did not know. Owing to the general vivacity of his character he invested the driest subjects with interest. Those who listened to his elocution almost fancied that they understood the highest things he talked about.

Mr. Lester F. Ward made a communication on the

ANIMAL POPULATION OF THE GLOBE.

He stated that he had recently had occasion to compile, chiefly from official sources, the statistics of live stock in the various countries of the globe from which any data could be obtained, and thought that some of the general results arrived at might possess sufficient scientific interest to warrant laying them before the Society.

The whole number of countries from which information of this character had been collated was twenty-seven, embracing all the countries of Europe except European Turkey, the several British Colonies in Australasia, the Island of Ceylon, Cape Colony and Natal in South Africa, Mauritius, the Dominion of Canada, Newfoundland, Jamaica, the Argentine Republic, Uruguay, Chili, and the United States. The species of animals of which cognizance was alone taken were: horses, mules, asses, horned cattle, sheep, goats, hogs, buffaloes, and reindeer. The reports were very incomplete except with respect to the four leading species, viz: horses, cattle, sheep, and hogs.

The total number of each species actually reported upon was as follows:

Horses - - - - - - -	47,181,384
Mules - - - - - - -	3,474,391
Asses - - - - - - -	2,217,166
Mules and asses, not distinguished -	11,849

Horned cattle - - - -	157,598,521
Sheep - - - - - - -	382,763,015
Goats - - - - - - -	15,704,911
Hogs - - - - - - -	81,691,331
Buffaloes - - - - -	89,281
Reindeer - - - - -	96,567
	690,828,416

The only species for which an estimate had been made of the total number in the world was the sheep. Mr. Robert P. Porter had made such an estimate, which, though varying from the official data in many of the above countries, afforded a basis for extending the figures already obtained to the remaining portions of the globe, and according to which the ovine population of the earth would reach 577,763,015. Using this result as a basis, a very rough estimate of the number of each of the remaining species in regions not already covered by actual enumerations would place the aggregate number of all the species named throughout the world at a little upward of one billion head and their distribution would then be about as follows:

Horses - - - - - - -	70,770,597
Cattle - - - - - - -	236,397,781
Sheep - - - - - - -	577,763,015
Hogs - - - - - - -	100,000,000
All other animals - - - -	32,391,247
	1,017,322,640

Reasons were, however, given for regarding this estimate considerably too low, both as to the number of sheep, upon which it is based, and also in the aggregate, and the speaker thought that the latter would probably reach nearly a billion and a half.

Comparisons were then made with the human population. According to a recent work by Baron Kolb the population of the 27 countries, from which reports were obtained, amounted, in 1878, to 366,100,000. This would give, upon an average, in all these countries, 130 horses, 430 cattle, 1,046 sheep, 224 hogs, and 29 of all the remaining animals taken together, to each 1,000 human beings, and for all these species combined, 1,887 animals to each 1,000 of population.

The latest issue of Behm & Wagner's *Bevölkerung der Erde*, (No. 6,) gives the present population of the earth at 1,456,000,000. If the above estimates of the number of each of these classes of animals in the entire world could be relied upon, they would show, for each 1,000 of human population, 50 horses, 166 cattle, 407 sheep, 70 hogs, and 23 of the other species taken together, or 716 of all the kinds enumerated. But, as above stated, these figures are probably far too low, and, if the truth could be known, it would probably be found that the animal population within these limits would not fall far below the human population.

The paper was concluded with some general observations on the moral bearings of the question of animal domestication. It was held that these facts constituted a sufficient justification of man's general treatment of the brute creation; that a larger amount of animal life exists under man's influence than could exist without it; that he creates more life than he destroys; that his methods of destruction are less painful than those of Nature; that it is to his interest to treat animals well, to supply them with abundant food, and relieve them from those constant fears, both of enemies and of want, which characterize their condition in a wild state; and that when life is taken, it is done quickly and as painlessly as possible; that the reverse of all this is the case in Nature, and hence a great amount of human sympathy is wasted on the creatures under man's control in consequence of ignorance of a few facts and principles.

Observations on the foregoing paper were made by Messrs. ELLIOTT and GILL.

The meeting then adjourned.

187TH MEETING. 10TH ANNUAL MEETING, NOVEMBER 6TH, 1880.

Vice-President HILGARD in the Chair.

Thirty-nine members present.

Meeting called to order by the Chair.

The Secretary read proceedings of the last annual meeting (168th meeting) held November 16th, 1879.

The names of members elected since the last annual meeting were announced.

Preliminary to voting, the list of paid up members was read.

The election of officers for the ensuing year was conducted in accordance with the rules of the Society, with the following results:—

President,	JOSEPH JANVIER WOODWARD.
Vice-Presidents,	W. B. TAYLOR, J. C. WELLING,
	J. E. HILGARD, J. K. BARNES,
Treasurer,	CLEVELAND ABBE.
Secretaries,	T. N. GILL, C. E. DUTTON.

MEMBERS OF THE GENERAL COMMITTEE.

JOHN W. POWELL,	SIMON NEWCOMB,
WILLIAM HARKNESS,	E. B. ELLIOTT,
GARRICK MALLERY,	CHAS. A. SHOTT,
JOHN R. EASTMAN,	THOMAS ANTISELL,

JOS. M. TONER.

It was moved by Mr. COFFIN—

That the consideration of the subject of annual reports to be made by the officers of the Society, be referred to the General Committee, for such action as they may deem desirable.

Adopted.

It was also moved by Mr. COFFIN—

That the General Committee be requested to provide some means for obtaining an annual address from the retiring President and report the same to the Society.

Adopted.

Society then adjourned.

188TH MEETING. NOVEMBER 20, 1880.

The President, Mr. J. J. WOODWARD, in the Chair, and 58 members present.

The newly-elected President addressed a few remarks to the Society, expressive of his high appreciation of the honor conferred upon him by his election as President of the Society, and conveying assurance of his desire and earnest efforts to fill the office acceptably, and to aid in rendering its meetings interesting and instructive.

The Chair announced the appointment of a Committee on Communications, viz: Mr. C. E. DUTTON and Mr. GARRICK MALLERY.

Mr. J. C. WELLING then presented, pursuant to a resolution of the Society passed at its 186th meeting, the following preamble and resolution relative to the decease of an honored fellow member, viz., the late General ALBERT J. MYER:

WHEREAS in the death of Brigadier General ALBERT J. MYER, late Chief Signal Officer of the Army, this Society has been called upon to mourn the loss of one of its founders as well as one of its most distinguished members, therefore, be it

Resolved, That in testifying our deep regret at the sudden termination of the useful life of General MYER, while as yet he was apparently in the mid-career of his activity, we, at the same time, would record our admiration of those energetic qualities which he brought to every sphere of duty he was called to fill, and by virtue of which he was able, on the one hand, to organize a system of military signaling highly valuable to the Government in the late war, and, on the other hand, to develop a wide field of usefulness by directing the whole energy of the signal service to the study and the practical applications of the science of meteorology, in both which provinces he displayed a remarkable talent for control and great liberality of public spirit.

Resolved, That these proceedings be entered upon the minutes of the Society.

The first communication of the evening was by Mr. JOHN JAY KNOX, entitled

THE DISTRIBUTION OF LOANS IN THE BANK OF FRANCE, THE NATIONAL BANKS OF THE UNITED STATES, AND THE IMPERIAL BANK OF GERMANY.

Mr. KNOX first gave a brief outline of the operations of the Bank of France during and since the late Franco-Prussian war. While it appears that the bank deals in very large amounts of money, particular attention was drawn to the fact that it also distributes among the people smaller amounts than the smallest banks in this country, and, in its annual reports of its transactions, prides itself upon the fact that it has rendered services to so many of the humblest citizens. After reciting the amount of commercial paper discounted, the amount of advances on collateral securities, and the amount of securities of the French Government held by it, he proceeded to quote from the bank reports of 1879 the classification of the Paris bills received at the bank:

Bills of 10 fr., or $2 each, and under - -	7,842
Bills of 11 fr. to 50 fr. each, or $2.20 to $10,	392,845
Bills of 51 fr. to 100 fr. each, or $10.20 to $20,	623,232
Bills of above 100 fr. each, or $20 - -	2,878,294
Total - - - - - - -	3,902,213

The average value of the bills thus discounted at Paris, in 1879, was 859 francs or $171.80. At the branches of the bank, of which there are ninety, the average amount of the bills discounted was 992 francs or $198.40. Similarly in the year 1878, this average value was, at Paris, 892 francs or $178.40, and in the branches of the bank 992 francs or 198.40. The averages for both the bank and its branches were for 1878, 944 francs or $188.80, and for 1879, 900 francs or $180.00.

The bank of France receives these bills from bankers who keep accounts with it as it discounts only for its depositors. These bankers in turn discount them for small brokers who receive them for this purpose from the working classes. The bills are presented at the bank with accompanying schedules. The rate of interest is the same on small bills as on large ones, and no charge is made beyond this ordinary discount or interest. The greater part of these small bills are promissory notes and issued from small manufacturers, and also from workmen on their own account, known as makers of the *Articles de Paris*. The annual exports of such articles amount it is said to twenty-five millions of dollars, and they consist of nic-nacs, toys, dolls, cheap bronze jewelry, and similar products.

Mr. KNOX also gave a classification of the notes and bills discounted and held by the National Banks of the United States on October, 2. 1879, when the total amount of loans was $875,013,107.

Geographical Divisions.	Number of banks.	$100 and less.	Over $100 and less than $500.	$500 and over, but less than $1,000.	$1,000 and over, but less than $5,000.	$5,000 and over, but less than $10,000.	$10,000 and over.	Total.	Amount.	Average.
New England States	547	80,167	54,965	20,444	33,621	10,082	4,590	153,869	$240,552,893 63	$1,563 36
Middle States	641	115,285	132,032	39,484	50,854	11,453	5,276	354,384	416,600,226 30	1,175 56
Southern States	175	15,752	24,480	7,862	8,936	1,288	416	58,729	45,890,807 95	781 40
Western States and Territories	685	90,141	8[illegible],563	27,590	31,812	5,381	1,800	241,287	171,969,179 22	712 72
United States	2,048	251,345	296,040	95,382	125,223	28,199	12,081	808,269	$875,013,107 10	$1,082 58

The number of pieces of paper discounted, as will be seen, was 808,269, and the average of each discount, $1,082,59. If the averrage time of these bills was sixty days, and the banks held continually the same amount, the number of discounts made during the year would be nearly five millions (4,849,614), the total discounts more than five thousand millions (5,250,000,000), which would be equal to a discount of $700 annually for each voter, or $500 for each family in the country. The number of notes and bills of $100 each or less at the date named was 251,345, or nearly one-third of the whole; the number of bills of less than $500 each was 547,385, or considerably more than two-thirds of the whole; while the number of bills of less than $1,000 each was 642,765, which is more than three-fourths of the whole number.

Among the States having the smallest average loans were the following: New York, exclusive of the cities of New York and Albany, $499; Pennsylvania, exclusive of Philadelphia and Pittsburgh, $566; Maryland, exclusive of Baltimore, $505; Kansas, in which the average was $353; Iowa, with an average of $375; West Virginia, of $350; Delaware, $556; New Jersey, $566; Minnesota, $621; Vermont, $645; North Carolina, $667; Tennessee, $651; Maine, $740; Indiana, $711; New Hampshire, $815; South Carolina, $846; Georgia, $882.

The Imperial Bank of Germany has a capital of $30,000,000, and is located in the city of Berlin.

The total number of bills of all kinds discounted during the year 1879 was 2,374,394, amounting to $852,175,650; the average amount of each bill being $358.90. The bills are classified as follows: There were 533,564 town bills, amounting to $263,663,280—average $494.15 each; the number of bills on places in Germany was 1,834,351, amounting to $578,693,335, and averaging $315.47 each; and the number of foreign bills was 6,479, in amount $9,819,035, and averaging $1,515.52 each. The average amount of loans and discounts for the year was $82,073,500.

Mr. E. B. Elliott inquired whether it is desirable that bills of such small amounts as those discounted by the Bank of France should be discounted in this country; if so, what plan could be suggested?

Mr. Knox replied that the savings banks, which receive deposits from all classes and in small amounts, might make small loans.

The laws restrict their investments to the best classes of securities. If there is any class oppressed by the want of loans it is poor people. They have a little money or negotiable property laid aside, upon which they frequently want to borrow, but they find nobody willing to loan upon it. Their only resource is to go to the note shavers and curbstone brokers, who charge them an exorbitant interest. Their wants, in his opinion, could be met by the savings banks.

Mr. J. J. WOODWARD read a communication entitled

RIDDELL'S BINOCULAR MICROSCOPES.—AN HISTORICAL NOTICE,

which is printed in full in the American Monthly Microscopical Journal for December, 1880.

[Abstract.]

Mr. WOODWARD exhibited a large binocular microscope, which he stated had been made for the late Dr. J. L. Riddell, then Professor of Chemistry in the University of Louisiana, during the winter of 1853–4 by the Grunow Brothers, of New Haven, Connecticut, and presented to the Army Medical Museum in April, 1879, by Dr. Riddell's widow.

He said that, although the proper merit of Riddell as a discoverer in this connection had been duly acknowledged by such high continental authorities as Harting and Frey, and even by some English writers, it had been strangely ignored by others, and that even so fair and usually so accurate an author as Dr. Wm. B. Carpenter had fallen into the error of asserting that "the first really satisfactory solution of the problem was that worked out by M. Nachet;" an error the more remarkable in view of the manner in which Riddell's discovery was published and discussed in England, and of the manner in which it had been used by the opticians of that country.

Mr. WOODWARD then offered evidence to show that Riddell was the first to discover and publish the optical principle on which all the really satisfactory binocular microscopes made prior to the present year depend, as well as the inventor of two efficient and still much employed methods of applying that principle; one suitable for the simple or dissecting microscope, the other for the compound microscope.

Riddell's discovery was, briefly, that the cone of rays proceeding from a single objective may be so divided by means of reflecting prisms, placed as close behind the posterior combination of the objective as possible, that orthoscopic binocular vision can be obtained both with the simple and the compound microscope. This discovery, together with an account of one method of carrying it out, and a suggestion of the feasibility of other methods, was published by Riddell in the New Orleans Monthly Medical Register for October, 1852, p. 4, and subsequently in the American Journal of Science and Arts, for January, 1853, p. 68. This article was reprinted in London, in the Quarterly Journal of Microscopical Science for April 1853. (Vol. I, 1853, p. 236.)

The contrivance described in this first paper was found by Riddell to give orthoscopic binocular vision when used without eye-pieces, but when ordinary eye-pieces were employed a pseudoscopic effect was obtained. This he obviated by the use of erecting eye-pieces; but, soon after his first paper was published, Riddell devised a second plan, which gave orthoscopic binocular vision with ordinary eye-pieces, and which he subsequently always used for the compound microscope, reserving his first plan for the dissecting (simple) microscope.

A brief notice, containing, however, a correct description of Riddell's second plan, was published in the New Orleans Monthly Medical Register for April, 1853, (p. 78,) and reprinted in London in the Quarterly Journal of Microscopical Science, Vol. I, 1853, (p. 304.) Subsequently, July 30, 1853, Riddell exhibited a dissecting (simple) microscope on his old plan and a compound microscope on his new plan to the American Association for the Advancement of Science, and read a paper describing those instruments, and pretty fully discussing the principles involved. This paper was published in the Proceedings of the Association, Vol. VII, for 1853, (p. 16,) and in the New Orleans Medical and Surgical Journal for November, 1853, (p. 321.) It was reprinted in London in the Quarterly Journal of Microscopical Science for January, 1854, Vol. II, (p. 18.)

Mr. Woodward then related the manner in which Riddell's discovery was discussed at the time, in England, by Messrs. Wheatstone and Wenham, and on the continent by M. M. Harting and Nachet. He showed that Nachet's modification of the compound microscope was suggested by Riddell's first instrument, and that Nachet's excellent binocular dissecting (simple) microscope is, in its optical parts, a literal copy of the binocular dissecting (simple) microscope exhibited by Riddell at the Cleveland meeting in July,

1853. This is also true of the binocular dissecting microscopes made of late years by Beck, of London, while the highly lauded erecting binocular microscope of Mr. J. W. Stephenson, F. R. M. S., (1870–72,) is, in its optical parts, a copy of the binocular compound microscope exhibited by Riddell at the Cleveland meeting. The latter instrument, as then exhibited, although optically efficient, was roughly put together by Riddell's own hands. The instrument exhibited by Mr. Woodward was ordered by Riddell of the Grunow Brothers, in August, 1853, and delivered to him by them in March following. In its optical parts it is a copy of the model exhibited at the Cleveland meeting, but some improvements were made in the mechanical details of its construction.

J. S. BILLINGS then made some remarks upon

THE SCIENTIFIC WORK CARRIED ON UNDER THE DIRECTION OF THE NATIONAL BOARD OF HEALTH.

Prof. Ira Remsen, of the Johns Hopkins University, has made for the Board an investigation on the organic matter in the air. By the use of tubes, filled with prepared pumice stone, all the nitrogenous matter in the air to be examined, was removed, and its quantity determined by the usual tests for free and albuminoid ammonia.

Air contaminated by being drawn through water containing decaying meat does not yield more than the usual quantity of albuminoid ammonia.

Air contaminated by being drawn over comparatively dry decaying organic matter yields more than the usual quantity of albuminoid ammonia.

Air contaminated by respiration yields more than the usual quantity of albuminoid ammonia.

The simple statement of fact that a given sample of air yields an abnormally large quantity of albuminoid ammonia is not sufficient to enable us to draw a conclusion with reference to the purity of the air. We must know at what season of the year the air was collected, and whether in the city or country; in fact, we should know everything possible concerning the air, and then let the conclusion finally drawn be a resultant of all the facts. It is probable, however, from what is now known, that the determination of the amount of albuminoid ammonia yielded by air may, under many circumstances, furnish us with important information concerning the quality of the air, but great caution is necessary in dealing with this principle of examination.

A series of investigations upon the effects of various soils upon ordinary sewage has been carried on under the direction of Prof. Pumpelly, of the United States Geological Survey, assisted by Prof. Smythe. The preliminary experiments related to the removal of living organisms from air and fluids by passing these through filters of various kinds, and then testing their effects upon solutions containing organic matter and susceptible of fermentative or putrefactive changes. A very large number of such solutions have been prepared and preserved under various conditions, and in no case has anything like fermentation or the development of the lower organisms been observed, unless under circumstances where the lower organisms could be introduced from without, thus giving strong negative evidence against the theory of spontaneous generation. The filtration of air from such germs was found to be a comparatively easy matter. Passing it through an inch of fine sand deprived it of the power of producing fermentative changes. On the other hand, the removal of bacteroidal organisms from water was much more difficult, filtration through many feet of fine sand being insufficient to effect it. The results reported by Wernich are confirmed, viz., that air passing over putrefying fluids or moist putrefying surfaces does not take up organisms therefrom, nor does it become contaminated by passing over dried bacteria films on smooth compact surfaces such as glass or iron. From woven stuffs, however, it is readily contaminated, and wherever there is dust there is danger.

The results obtained by Dr. Bigelow in attempting to destroy the vitality of dried bacteria films by means of gaseous disinfectants were then mentioned. It is found that time is an important element in the matter, and that long exposures are necessary to secure complete destruction of vitality of such organisms. This may explain the failures to disinfect the Plymouth and the Excelsior by gaseous disinfectants.

Drs. H. C. Wood and H. F. Fremont have made a number of experiments on the inoculation of diphtheria on the lower animals with negative results. The theory of Oertel that this disease is due to specific bacteria is not confirmed by their observations. They state that their results seem to indicate that the contageous material of diphtheria is of the nature of a septic poison which is locally very irritating to the mucous membrane, and that the disease may be often a purely local affection to be treated by local remedies.

Dr. G. M. Sternberg has been repeating the experiments of Klebs and Tommasi-Crudelli on the bacillus malariæ. He finds in the malarious swamps around New Orleans, organisms not distinguishable from those figured by the authors referred to, and on cultivating them in gelatin solutions obtains a similar bacillus. He has not however obtained any specific effects by injecting these organisms into the blood of animals and is unable to confirm the conclusions announced by Klebs.

Dr. Chas. Smart, U. S. A., has been engaged on water analysis, and for the last seven months on the adulterations of food. From an analysis of over six hundred samples he concludes that while there is a considerable amount of adulteration in such articles as ground coffee and spices there is not much that is dangerous to health—in the words of the last British Parliamentary Commission we are cheated but not poisoned. Poisonous colors derived from lead and antimony are found in some candies.

The educational work of the Board was then referred to, and more especially its efforts to secure a uniform and satisfactory mode of reporting mortality statistics.

At the conclusion of Mr. Billings' remarks the society adjourned.

189TH MEETING DECEMBER 4, 1880.

The President in the chair.

Forty-eight members present.

The minutes of the last meeting were read and adopted.

The Chair announced to the Society the election and acceptance of the following new members: ALEXANDER SMYTHE CHRISTIE, WILLIAM CRAWFORD WINLOCK, and WINSLOW UPTON.

The Chair also announced the appointment of Mr. WILLIAM HARKNESS as an additional member of the Standing Committee on Communications.

The Society then listened to the address of the retiring President, Mr. Simon Newcomb, on

THE RELATION OF SCIENTIFIC METHOD TO SOCIAL PROGRESS.

Among those subjects which are not always correctly apprehended, even by educated men, we may place that of the true significance of scientific method, and the relations of such method to practical affairs. This is especially apt to be the case in a country like our own, where the points of contact between the scientific world on the one hand, and the industrial and political world on the other, are fewer than in other civilized countries. The form which this misapprehension usually takes is that of a failure to appreciate the character of scientific method, and especially its analogy to the methods of practical life. In the judgment of the ordinary intelligent man there is a wide distinction between theoretical and practical science. The latter he considers as that science directly applicable to the building of railroads, the construction of engines, the invention of new machinery, the construction of maps, and other useful objects. The former he considers analogous to those philosophic speculations in which men have indulged in all ages without leading to any result which he considers practical. That our knowledge of nature is increased by its prosecution is a fact of which he is quite conscious, but he considers it as terminating with a mere increase of knowledge, and not as having in its method anything which a person devoted to material interests can be expected to appreciate.

This view is strengthened by the spirit with which he sees scientific investigation prosecuted. It is well understood on all sides that when such investigations are pursued in a spirit really recognized as scientific, no merely utilitarian object is had in view. Indeed it is easy to see how the very fact of pursuing such an object would detract from that thoroughness of examination which is the first condition of a real advance. True science demands in its every research a completeness far beyond what is apparently necessary for its practical applications. The precision with which the astronomer seeks to measure the heavens, and the chemist to determine the relations of the ultimate molecules of matter has no limit, except that set by the imperfections of the instruments of research. There is no such division recognized as that of useful and useless knowledge. The ultimate aim is nothing less than that of bringing all the phenomena of nature under laws as exact as those which govern the planetary motions.

Now the pursuit of any high object in this spirit commands from men of wide views that respect which is felt towards all exertion having in view more elevated objects than the pursuit of gain. Accordingly it is very natural to classify scientists, and philosophers with the men who in all ages have sought after learning instead of utility. But there is another aspect of the question which will show the relations of scientific advance to the practical affairs of life in a different light. I make bold to say that the greatest want of the day, from a purely practical point of view, is the more general introduction of the scientific method and the scientific spirit into the discussion of those political and social problems which we encounter on our road to a higher plane of public well being. Far from using methods too refined for practical purposes, what most distinguishes scientific from other thought is the introduction of the methods of practical life into the discussion of abstract general problems. A single instance will illustrate the lesson I wish to enforce.

The question of the tariff is, from a practical point of view, one of the most important with which our legislators will have to deal during the next few years. The widest diversity of opinion exists as to the best policy to be pursued in collecting a revenue from imports. Opposing interests contend against each other without any common basis of fact or principle on which a conclusion can be reached. The opinions of intelligent men differ almost as widely as those of the men who are immediately interested. But all will admit that public action in this direction should be dictated by one guiding principle—that the greatest good of the community is to be sought after. That policy is the best which will most promote this good. Nor is there any serious difference of opinion as to the nature of the good to be had in view; it is in a word the increase of the national wealth and prosperity. The question on which opinions fundamentally differ is that of the effects of a higher or lower rate of duty upon the interests of the public. If it were possible to foresee, with an approach to certainty, what effect a given tariff would have upon the producers and consumers of an article taxed, and, indirectly, upon each member of the community in any way interested in the article, we should then have an exact datum which.we do not now possess for reaching a conclusion. If some superhuman authority, speaking with the voice of infallibility, could give us this information, it is evident that a great national want would be supplied. No question in practical life is more important than this: How can this desirable knowledge of the economic effects of a tariff be obtained?

The answer to this question is clear and simple. The subject must be studied in the same spirit, and, to a certain extent, by the same methods which have been so successful in advancing our knowledge of nature. Every one knows that, within the last two centuries, a method of studying the course of nature has been introduced which has been so successful in enabling us to trace the sequence of cause and effect as almost to revolutionize society. The very fact that scientific method has been so successful here leads to the belief that it might be equally successful in other departments of inquiry.

The same remarks will apply to the questions connected with banking and currency; the standard of value; and, indeed, all subjects which have a financial bearing. On every such question we see wide differences of opinion without any common basis to rest upon.

It may be said, in reply, that in these cases there are really no grounds for forming an opinion, and that the contests which arise over them are merely those between conflicting interests. But this claim is not at all consonant with the form which we see the discussion assume. Nearly every one has a decided opinion on these several subjects; whereas, if there were no data for forming an opinion, it would be unreasonable to maintain any whatever. Indeed, it is evident that there must be truth somewhere, and the only question that can be open is that of the mode of discovering it. No man imbued with a scientific spirit can claim that such truth is beyond the power of the human intellect. He may doubt his own ability to grasp it, but cannot doubt that by pursuing the proper method and adopting the best means the problem can be solved. It is, in fact, difficult to show why some exact results could not be as certainly reached in economic questions as in those of physical science. It is true that if we pursue the inquiry far enough we shall find more complex conditions to encounter, because the future course of demand and supply enters as an uncertain element. But a remarkable fact to be considered is that the difference of opinion to which we allude does not depend upon different estimates of the future, but upon different views of the most elementary and general principles of the subject. It is as if men were not agreed whether air were elastic or whether the earth turns on its axis. Why is it that while in all subjects of physical science we find a general agreement through a wide range of subjects, and doubt commences only where certainty is not attained, yet when we turn to economic subjects we do not find the beginning of an agreement?

No two answers can be given. It is because the two classes of subjects are investigated by different instruments and in a different spirit. The physicist has an exact nomenclature; uses methods of research well adapted to the objects he has in view; pursues his investigations without being attacked by those who wish for different results; and, above all, pursues them only for the purpose of discovering the truth. In economical questions the case is entirely different. Only in rare cases are they studied without at least the suspicion that the student has a preconceived theory to support. If results are attained which oppose any powerful interest, this interest can hire a competing investigator to bring out a different result. So far as the public can see, one man's result is as good as another's, and thus the object is as far off as ever. We may be sure that until there is an intelligent and rational public, able to distinguish between the speculations of the charlatan and the researches of the investigator, the present state of things will continue. What we want is so wide a diffusion of scientific ideas that there shall be a class of men engaged in studying economical problems for their own sake, and an intelligent public able to judge what they are doing. There must be an improvement in the objects at which they aim in education, and it is now worth while to inquire what that improvement is.

It is not mere instruction in any branch of technical science that is wanted. No knowledge of chemistry, physics, or biology, however extensive, can give the learner much aid in forming a correct opinion of such a question as that of the currency. If we should claim that political economy ought to be more extensively studied, we would be met by the question, which of several conflicting systems shall we teach? What is wanted is not to teach this system or that, but to give such a training that the student shall be able to decide for himself which system is right.

It seems to me that the true educational want is ignored both by those who advocate a classical and those who advocate a scientific education. What is really wanted is to train the intellectual powers, and the question ought to be, what is the best method of doing this? Perhaps it might be found that both of the conflicting methods could be improved upon. The really distinctive features, which we should desire to see introduced, are two in number: the one the scientific spirit; the other the scientific discipline. Although many details may be classified under each of these heads, yet there is one of pre-eminent importance on which we should insist.

The one feature of the scientific spirit which outweighs all others in importance is the love of knowledge for its own sake. If by our system of education we can inculcate this sentiment we shall do what is, from a public point of view, worth more than any amount of technical knowledge, because we shall lay the foundation of all knowledge. So long as men study only what they think is going to be useful their knowledge will be partial and insufficient. I think it is to the constant inculcation of this fact by experience, rather than to any reasoning, that is due the continued appreciation of a liberal education. Every business man knows that a business-college training is of very little account in enabling one to fight the battle of life, and that college bred men have a great advantage even in fields where mere education is a secondary matter. We are accustomed to seeing ridicule thrown upon the questions sometimes asked of candidates for the civil service because the questions refer to subjects of which a knowledge is not essential. The reply to all criticisms of this kind is that there is no one quality which more certainly assures a man's usefulness to society than the propensity to acquire useless knowledge. Most of our citizens take a wide interest in public affairs, else our form of government would be a failure. But it is desirable that their study of public measures should be more critical and take a wider range. It is especially desirable that the conclusions to which they are led should be unaffected by partisan sympathies. The more strongly the love of mere truth is inculcated in their nature the better this end will be attained.

The scientific discipline to which I ask mainly to call your attention consists in training the scholar to the scientific use of language. Although whole volumes may be written on the logic of science there is one general feature of its method which is of fundamental significance. It is that every term which it uses and every proposition which it enunciates has a precise meaning which can be made evident by proper definitions. This general principle of scientific language is much more easily inculcated by example than subject to exact description; but I shall ask leave to add one to several attempts I have made to define it. If I should say that when a statement is made in the language of science the speaker knows what he means, and the hearer either knows it or can be made to know it by proper definitions, and that this community of understanding is frequently not reached in other departments of thought, I might be understood as casting a slur on whole departments of inquiry. Without intending any such slur, I may still say that language and statements are worthy of the name scientific as they approach this standard; and, moreover, that a great deal is said and written which does not fulfill the requirement. The fact that words lose their meaning when removed from the connections in which that meaning has been acquired and put to higher uses, is one which, I think, is rarely recognized. There is nothing in the history of philosophical inquiry more curious than the frequency of interminable disputes on subjects where no agreement can be reached because the opposing parties do not use words in the same sense. That the history of science is not free from this reproach is shown by the fact of the long dispute whether the force of a moving body was proportional to the simple velocity or to its square. Neither of the parties to the dispute thought it worth while to define what they meant by the word "force," and it was at length found that if a definition was agreed upon the seeming difference of opinion would vanish. Perhaps the most striking feature of the case, and one peculiar to a scientific dispute, was that the opposing parties did not differ in their solution of a single mechanical problem. I say this is curious, because the very fact of their agreeing upon every concrete question which could have been presented, ought to have made it clear that some fallacy was lacking in the discussion as to the measure of force. The good effect of a scientific spirit is shown by the fact that this discussion is almost unique in the history of science during the past two centuries, and that scientific men themselves were able to see the fallacy involved, and thus to bring the matter to a conclusion.

If we now turn to the discussions of philosophers, we shall find at least one yet more striking example of the same kind. The question of the freedom of the human will has, I believe, raged for centuries. It cannot yet be said that any conclusion has been reached. Indeed I have heard it admitted by men of high intellectual attainments that the question was insoluble. Now a curious feature of this dispute is that none of the combatants, at least on the affirmative side, have made any serious attempt to define what should be meant by the phrase freedom of the will, except by using such terms as require definition equally with the word freedom itself. It can, I conceive, be made quite clear that the assertion, "The will is free," is one without meaning, until we analyze more fully the different meanings to be attached to the word free. Now this word has a perfectly well-defined signification in every day life. We say that anything is free when it is not subject to external constraint. We also know exactly what we mean when we say that a man is free to do a certain act. We mean that if he chooses to do it there is no external constraint acting to prevent him. In all cases a relation of two things is implied in the word, some active agent or power, and the presence or absence of another constraining agent. Now, when we inquire whether the will itself is free, irrespective of external constraints, the word free no longer has a meaning, because one of the elements implied in it is ignored.

To inquire whether the will itself is free is like inquiring whether fire itself is consumed by the burning, or whether clothing is itself clad. It is not, therefore, at all surprising that both parties have been able to dispute without end, but it is a most astonishing phenomenon of the human intellect that the dispute should go on generation after generation without the parties finding out whether there was really any difference of opinion between them on the subject. I venture to say that if there is any such difference, neither party has ever analyzed the meaning of the words used sufficiently far to show it. The daily experience of every man, from his cradle to his grave, shows that human acts are as much the subject of external causal influences as are the phenomena of nature. To dispute this would be little short of the ludicrous. All that the opponents of freedom, as a class, have ever claimed, is the assertion of a causal connection between the acts of the will, and influences independent of the will. True, propositions of this sort can be expressed in a variety of ways connoting an endless number of more or less objectionable ideas, but this is the substance of the matter.

To suppose that the advocates on the other side meant to take issue on this proposition would be to assume that they did not know what they were saying. The conclusion forced upon us is that though men spend their whole lives in the study of the most elevated department of human thought it does not guard them against the danger of using words without meaning. It would be a mark of ignorance, rather than of penetration, to hastily denounce propositions on subjects we are not well acquainted with because we do not understand their meaning. I do not mean to intimate that philosophy itself is subject to this reproach. When we see a philosophical proposition, couched in terms we do not understand, the most modest and charitable view is to assume that this arises from our lack of knowledge. Nothing is easier than for the ignorant to ridicule the propositions of the learned. And yet, with every reserve, I cannot but feel that the disputes to which I have alluded prove the necessity of bringing scientific precision of language into every demand of thought. If the discussion had been confined to a few, and other philosophers had analyzed the subject, and showed the fictitious character of the discussion, or had pointed out where opinions really might differ, there would be nothing derogatory to philosophers. But the most suggestive circumstance is that although a large proportion of the philosophic writers in recent times have devoted more or less attention to the subject, few, or none, have made even this modest contribution. I speak with some little confidence on this subject, because several years ago I wrote to one of the most acute thinkers of the country, asking if he could find in philosophical literature any terms or definitions expressive of the three different senses in which not only the word freedom, but nearly all words implying freedom were used. His search was in vain.

Nothing of this sort occurs in the practical affairs of life. All terms used in business, however general or abstract, have that well-defined meaning which is the first requisite of the scientific language. Now one important lesson which I wish to inculcate is that the language of science in this respect corresponds to that of business; in that each and every term that is employed has a meaning as well defined as the subject of discussion can admit of. It will be an instructive exercise to inquire what this peculiarity of scientific and business language is. It can be shown that a certain requirement should be fulfilled by all language intended for the discovery of truth, which is fulfilled only by the two classes of language which I have described. It is one of the most common errors of discourse to assume that any common expression which we may use always conveys an idea, no matter what the subject of discourse. The true state of the case can, perhaps, best be seen by beginning at the foundation of things, and examining under what conditions language can really convey ideas.

Suppose thrown among us a person of well-developed intellect, but unacquainted with a single language or word that we use. It is absolutely useless to talk to him, because nothing that we say conveys any meaning to his mind. We can supply him no dictionary, because by hypothesis he knows no language to which we have access. How shall we proceed to communicate our ideas to him? Clearly there is but one possible way, namely, through his five senses. Outside of this means of bringing him in contact with us we can have no communication with him. We, therefore, begin by showing him sensible objects, and letting him understand that certain words which we use correspond to those objects. After he has thus acquired a small vocabulary, we make him understand that other terms refer to relations between objects which he can perceive by his senses. Next he learns, by induction, that there are terms which apply not to special objects, but to whole classes of objects. Continuing the same process, he learns that there are certain attributes of objects made known by the manner in which they affect his senses, to which abstract terms are applied. Having learned all this, we can teach him new words by combining words without exhibiting objects already known. Using these words we can proceed yet further, building up, as it were, a complete language. But there is one limit at every step. Every term which we make known to him must depend ultimately upon terms the meaning of which he has learned from their connection with special objects of sense.

To communicate to him a knowledge of words expressive of mental states it is necessary to assume that his own mind is subject to these states as well as our own, and that we can in some way indicate them by our acts. That the former hypothesis is sufficiently well established can be made evident so long as a consistency of different words and ideas is maintained. If no such consistency of meaning on his part were evident, it might indicate that the operations of his mind were so different from ours that no such communication of ideas was possible. Uncertainty in this respect must arise as soon as we go beyond those mental states which communicate themselves to the senses of others.

We now see that in order to communicate to our foreigner a knowledge of language, we must follow rules similar to those necessary for the stability of a building. The foundation of the building must be well laid upon objects knowable by his five senses. Of course the mind, as well as the external object, may be a factor in determining the ideas which the words are intended to express; but this does not in any manner invalidate the conditions which we impose. Whatever theory we may adopt of the relative part played by the knowing subject, and the external object in the acquirement of knowledge, it remains none the less true that no knowledge of the meaning of a word can be acquired except through the senses, and that the meaning is, therefore, limited by the senses. If we transgress the rule of founding each meaning upon meanings below it, and having the whole ultimately resting upon a sensuous foundation, we at once branch off into sound without sense. We may teach him the use of an extended vocabulary, to the terms of which he may apply ideas of his own, more or less vague, but there will be no way of deciding that he attaches the same meaning to these terms that we do.

What we have shown true of an intelligent foreigner is necessarily true of the growing man. We come into the world without a knowledge of the meaning of words, and can acquire such knowledge only by a process which we have found applicable to the intelligent foreigner. But to confine ourselves within these limits in the use of language requires a course of severe mental discipline. The transgression of the rule will naturally seem to the undisciplined mind a mark of intellectual vigor rather than the reverse. In our system of education every temptation is held out to the learner to transgress the rule by the fluent use of language to which it is doubtful if he himself attaches clear notions, and which he can never be certain suggests to his hearer the ideas which he intends. Indeed, we not infrequently see, even among practical educators, expressions of positive antipathy to scientific precision of language so obviously opposed to good sense that they can be attributed only to a failure to comprehend the meaning of the language which they criticise.

Perhaps the most injurious effect in this direction arises from the natural tendency of the mind, when not subject to a scientific discipline, to think of words expressing sensible objects and their relations as connoting certain supersensuous attributes. This is frequently seen in the repugnance of the metaphysical mind to receive a scientific statement about a matter of fact simply as a matter of fact. This repugnance does not generally arise in respect to the every day matters of life. When we say that the earth is round we state a truth which every one is willing to receive as final. If without denying that the earth was round, one should criticise the statement on the ground that it was not necessarily round but might be of some other form, we should simply smile at this use of language. But when we take a more general statement and assert that the laws of nature are inexorable, and that all phenomona, so far as we can show, occur in obedience to their requirements, we are met with a sort of criticism with which all of us are familiar, and which I am unable adequately to describe. No one denies that as a matter of fact, and as far as his experience extends, these laws do appear to be inexorable. I have never heard of any one professing, during the present generation, to describe a natural phenomenon, with the avowed belief that it was not a product of natural law; yet we constantly hear the scientific view criticised on the ground that events may occur without being subject to natural law. The word "may," in this connection, is one to which we can attach no meaning expressive of a sensuous relation.

This is, however, not the most frequent misuse of the word may. In fact, the unscientific use of language to which I refer, is most strongly shown in disquisitions on the freedom of the will. When I say that it is perfectly certain that I will to-morrow perform a certain act unless some cause external to my mind which I do not now foresee occurs to prevent me, I make a statement which is final so far as scientific ideas are concerned. But it will sometimes be maintained that however certain it may be that I shall perform this act, nevertheless I may act otherwise. All I can say to this is that I do not understand the meaning of the statement.

The analogous conflict between the scientific use of language and the use made by some philosophers, is found in connection with the idea of causation. Fundamentally the word cause is used in scientific language in the same sense as in the language of common life. When we discuss with our neighbors the cause of a fit of illness, of a fire, or of cold weather, not the slightest ambiguity attaches to the use of the word, because whatever meaning may be given to it is founded only on an accurate analysis of the ideas involved in it from daily use. No philosopher objects to the common meaning of the word, yet we frequently find men of eminence in the intellectual world who will not tolerate the scientific man in using the word in this way. In every explanation which he can give to its use they detect ambiguity. They insist that in any proper use of the term the idea of power must be connoted. But what meaning is here attached to the word power, and how shall we first reduce it to a sensible form, and then apply its meaning to the operations of nature? That this can be done, I by no means deny. All I maintain is that if we shall do it, we must pass without the domain of scientific statement.

Perhaps the greatest advantage in the use of symbolic and other mathematical language in scientific investigation is that it cannot possibly be made to connote anything except what the speaker means. It adheres to the subject matter of discourse with a tenacity which no criticism can overcome. In consequence, whenever a science is reduced to a mathematical form its conclusions are no longer the subject of philosophical attack. To secure the same desirable quality in all other scientific language it is necessary to give it, so far as possible, the same simplicity of signification which attaches to mathematical symbols. This is not easy, because we are obliged to use words of ordinary language, and it is impossible to divest them of whatever they may connote to ordinary hearers.

I have thus sought to make it clear that the language of science corresponds to that of ordinary life, and especially of business life, in confining its meaning to phenomena. An analogous statement may be made of the method and objects of scientific investigation. I think Professor Clifford was very happy in defining science as organized common sense. The foundation of its widest general creations is laid, not in any artificial theories, but in the natural beliefs and tendencies of the human mind. Its position against those who deny these generalizations is quite analogous to that taken by the Scottish school of philosophy against the skepticism of Hume.

It may be asked, if the methods and language of science correspond to those of practical life,—why is not the every day discipline of that life as good as the discipline of science? The answer is, that the power of transferring the modes of thought of common life to subjects of a higher order of generality is a rare faculty which can be acquired only by scientific discipline. What we want is that in public affairs men shall reason about questions of finance, trade, national wealth, legislation and administration with the same consciousness of the practical side that they reason about their own interests. When this habit is once acquired and appreciated, the scientific method will naturally be applied to the study of questions of social policy. When a scientific interest is taken in such questions, their boundaries will be extended beyond the utilities immediately involved, and then the last condition of unceasing progress will be complied with.

At the conclusion of Mr. Newcomb's address it was moved by Mr. Hilgard that the thanks of the Society are due to Mr. Newcomb for his weighty, instructive, and interesting address.

The motion was carried.

Mr. J. E. HILGARD then made a communication on the subject of

A MODEL OF THE BASIN OF THE GULF OF MEXICO.

He exhibited to the Society a model of the Gulf of Mexico recently constructed under the direction of the Coast Survey Office upon data obtained by a very great number of soundings. Of these many thousands have been made, and the model is believed to be very correct. As constructed, the vertical scale is thirty times as great as the horizontal in order to emphasize and render easily intelligible the most notable features.

The soundings of the waters in the Gulf of Mexico began with the extension thither of the work of the Coast Survey, but they were at first only littoral and tributary to the topographic and hydrographic work of the Bureau. They were interrupted by the civil war, but were resumed at its close. Soundings had also been made off the east coast of Florida to ascertain the nature and dimensions of the outlet of the Gulf stream. This outlet was found to be relatively quite small. Soundings and temperatures had been taken from Florida to Cuba and to Yucatan. Within a few years the work of exploring the general configuration of the Gulf of Mexico has been commenced by Commander Sigsbee, of the Navy, on duty in the Coast and Geodetic Survey. This officer made great improvements in deep-sea sounding apparatus, and, prosecuting the exploration with great energy and ingenuity, has brought the work to a speedy conclusion.

As a result of these investigations, it is found that the continental profiles which descend from every direction beneath the water of the gulf, have, at first, a very gradual slope of a few feet to the mile—until the 100 fathom depth, or thereabout, is reached. They then descend much more rapidly, and, in some places, with singular abruptness to depths exceeding 2,000 fathoms. All around the gulf shores is a marginal belt of varying width and of comparatively shallow water. Within this marginal belt is an area of similar shape to that of the gulf itself, and nearly concentric with its coast, where the depth is comparable to that of mid-ocean. The extent of the deeper area is about 50,000 square miles. It also appears that the continental or peninsular mass of Florida is of much greater area than that portion which exposes its surface above the water, and the same is true of Yucatan. An examination of the portions in the vicinity of the Mississippi river, shows that the delta has very nearly reached the position where the profile begins to drop rapidly down into deep water, and the apprehensions of those who fear that the jetties lately constructed may cause the accumulation of deposits further out may therefore be dispelled or greatly mitigated.

Turning to the channel of the Gulf stream, Mr. Hilgard remarked that its transverse section between Florida and the Bahama Banks, did not exceed twelve square miles. With an average current velocity of only 2½ miles per hour, it appears quite incredible that enough water can be dischaged through this passage to occasion the mild climate of western Europe. The main mass of the great oceanic drift which warms these shores, he thought must be derived from the Caribbean Sea, passing out between the greater Antilles, where the passes are far wider and deeper. Of this greater oceanic drift the efflux through the Florida straits forms but a small part.

Remarks upon this communication were made by Messrs. Alvord, Dutton, Gill, Harkness and White.

The Society then adjourned.

190th Meeting. December 18, 1880.

The President in the Chair.

Forty-two members present.

The minutes of the last meeting were read and adopted.

A communication was then read by Mr. SWAN M. BURNETT, entitled

COLOR PERCEPTION AND COLOR BLINDNESS.

The speaker first gave the Young-Helmholtz theory, which consists in the assumption of three fibres in the retina corresponding to the so-called fundamental colors, red, green and violet, stating the objections that have been brought against this theory by Mauthner and others, when viewed from the standpoint of color blindness.

He then explained in brief the theory of Prof. Hering, of Prague, according to which there are supposed to be in the retina three chemical substances, which are called the *black-white*, the *red-green*, and the *blue-yellow*. These are acted on by light, by assimilation, and by dissimilation. Dissimilation (D) of the black-white substance produces white, its assimilation (A) black. The D-action on the red-green produces red, the A-action green. The D-action on the blue-yellow substance produces blue, the A-action yellow. When one of the substances is lacking there is an inability to properly perceive the pair of colors peculiar to it. There is therefore red-green blindness, and blue-yellow blindness. The objections to this theory as advanced by Prof. Donders and others were then brought forward.

There are two strong objections to both these theories aside from those mentioned, first, their want of simplicity, and second, the necessity of inventing new tissues and novel reactions of tissues to the affecting agent.

The true theory of colors, when found, we have every right to expect will be simple, and the laws governing it will be in keeping with the action of light on simple substances, and in the opinion of the speaker, they would be found to lie in the direction of the recent discoveries of the action of light on the molecular structure of homogeneous substances, and he accepted as the foundation of his speculations that *variation in sensation would have its basis, not in complexity of tissue, but in the varying action of the affecting agent.*

A theory on this basis would have the retina a substance whose molecular structure would be such as to allow it to respond promptly to each of those undulations of the ether corresponding to the principal colors. The wave length corresponding to red, for example, would produce a molecular change (most probably simply vibratory) which would be carried to the brain centre of vision by the optic nerve, and there transformed into a distinct sensation. The same would hold good probably for the orange, yellow, green, blue and violet. We have an analogy for such reaction in the molecular change produced by light in the metal selenium when in a crystallized state, and in some other substances. The photophone depends for its existence upon this delicate reaction of the molecular structure of selenium to the influence of light. Which are the primary and which the secondary colors—that is those arising from mixed sensations—would have to be determined by experiment.

The speaker would divide color blindness into two classes, *peripheral* and *central*. In the former the retina and optic nerve would be the agents affected, in the latter the cerebral centre of vision. The latter he considered to be the most common form of congenital color blindness, and it was due in his opinion to the fact that this centre had not yet developed the power of properly differentiating the closely allied impressions sent to it. In such cases, the spectrum was not shortened, but was seen dichromic, the line of demarcation being usually at the blue.

As regards the *retinal* form one broad general principle might be laid down, that where there was a lacking color the molecular changes in the retina were such as to incapacitate it from responding promptly to the wave lengths which physically represent that color.

Believing that education had much to do with the development of the color-sense, the speaker had devised a plan for the "systematic education of the color-sense in children," which, if followed out closely, would, he believed, in the course of generations, make color-blindness as rare in the male sex as it now is among females. This plan is published in full in the Archives of Ophthalmology. (G. P. Putnam's Sons, New York, October, 1879.)

The next communication was by Mr. E. M. GALLAUDET, entitled—

THE INTERNATIONAL CONVENTION OF THE TEACHERS OF THE DEAF AND DUMB, AT MILAN.

Mr. GALLAUDET recited first certain resolutions adopted at that convention, which were as follows:

"The convention, considering the incontestable superiority of speech over signs, 1st, for restoring deaf-mutes to social life, 2d, for giving them greater facility of language, declares that the method of articulation should have the preference over that of signs in the instruction and education of the deaf and dumb.

"Considering that the simultaneous use of signs and speech has the disadvantage of injuring speech and lip reading and precision of ideas, the convention declares that the pure oral method ought to be preferred."

Apropos to these resolutions, Mr. GALLAUDET quoted the comments of the London *Times*, which journal remarks that—

"No more representative body could have been collected than that which at Milan has declared for oral teaching for the deaf and dumb, and for nothing but oral teaching," and also speaks of the action of the convention as expressing a "virtual unanimity of preference for oral teaching, which might seem to overbear all possibility of opposition."

Mr. GALLAUDET then proceeded to explain the composition of the convention, which, he stated, consisted of 164 members, of whom eighty-seven were Italians and fifty-six French, these two nationalities composing seven-eighths of its representation. There were from America five members, while the city of Milan alone furnished forty-six. The president and secretary, both oralists, were from Milan, and seven out of eight other officers were also oralists. The Paris convention, in 1878, had been organized by the Pereire Society, an active propaganda in favor of the exclusive oral method; and the organization of the Milan convention was of a similar nature, and cannot be regarded as representative of the general body of instructors of the deaf and dumb throughout the world, as the preceding statement of its composition must indicate. The American delegates voted in favor of the combined method of teaching, both orally and by signs.

He expressed, in closing, the conviction that teachers of this country are working in the right direction, and that, in due time, the relative importance as well as the proper sphere of the two methods will be fully recognized in the combined system.

191ST MEETING. JANUARY 8, 1881.

Vice-President TAYLOR in the Chair.

Twenty-seven members present.

The minutes of the last meeting were read and adopted.

A communication by Mr. W. F. McK. Ritter was then read, entitled—

ON A SIMPLE METHOD OF DERIVING SOME EQUATIONS USED IN THE THEORY OF THE MOON AND OF THE PLANETS.

The rectangular and polar co-ordinates of a heavenly body are functions of the elements of the orbit and of the time. When the elements are pure constants, as in the case of undisturbed motion, these co-ordinates vary only with the time; but when the effect of the disturbing force is considered, we have variation or perturbation of the elements, and hence, also, the co-ordinates vary both with the time and the elements.

Since the co-ordinates are functions of the elements, as long as the variations of the elements are unknown, the corresponding corrections to the co-ordinates, due to these variations, must be regarded as zero. Hence, in the differentiation, the differentials of the co-ordinates with respect to the elements, alone considered as variable, must be put equal to zero. Hence, also, the velocities of the rectangular and polar co-ordinates are zero, and thus we are furnished with equations of condition, which greatly facilitate the solution of the problem of determining the perturbations of the elements.

In finding what are called the special perturbations, we resolve the disturbing force into three components.

For this purpose, call

R, the component in the direction of the radius-vector,

S, the component perpendicular to the radius-vector, parallel to the plane of the orbit, and positive in the direction of the motion, and

Z, the component perpendicular to the plane of the orbit.

The values of these components, in the form we wish to employ, are

$$R = k^2 (1 + m) \quad \frac{d\Omega}{dr},$$

$$S = k^2 (1 + m) \frac{1}{r} \frac{d\Omega}{dv},$$

$$Z = k^2 (1 + m) \quad \frac{d\Omega}{dz}.$$

Here Ω is the disturbing function, r and v are polar co-ordinates, z the co-ordinate perpendicular to the plane of the orbit, k^2 the

Gaussian constant, and m the relation of the mass of the disturbed body to that of the sun.

By putting the first differential co-efficients of the co-ordinates with respect to the time equal to zero, we derive, with great ease, the expressions for the variations of the elements. This is for the case of special perturbations. These expressions will contain the components R, S, and Z.

If we now substitute the values of these components, wherever they appear, and perform the necessary reductions, we get expressions for the variations of the elements, where, instead of the components of the disturbing force, the force itself appears.

In the case of the mean anomaly, another method has been followed. Its variation can best be found by means of the relation

$$M = \mu (t - T),$$

where M represents the mean anomaly, μ the mean daily motion, and T the time of perihelion-passage.

I have thus derived, among others, the equations :

$$\frac{dL}{dt} = k^2 (1 + m) \frac{d\Omega}{dM}, \quad \frac{dM}{dt} = - k^2 (1 + m) \frac{d\Omega}{dL}$$

$$\frac{dG}{dt} = k^2 (1 + m) \frac{d\Omega}{d\omega}, \quad \frac{d\omega}{dt} = - k^2 (1 + m) \frac{d\Omega}{dG},$$

$$\frac{dH}{dt} = k^2 (1 + m) \frac{d\Omega}{d\Omega}, \quad \frac{d\Omega}{dt} = - k^2 (1 + m) \frac{d\Omega}{dH}.$$

From these, by slight changes, we get the equations used by Delaunay in his theory of the moon's motion. Thus by putting $k^2 (1 + m) \Omega = R$, and writing l, g, h, for M, ω, Ω, respectively, we have

$$\frac{dL}{dt} = \frac{dR}{dl}, \quad \frac{dl}{dt} = - \frac{dR}{dL},$$

$$\frac{dG}{dt} = \frac{dR}{dg}, \quad \frac{dg}{dt} = - \frac{dR}{dG},$$

$$\frac{dH}{dt} = \frac{dR}{dh}; \quad \frac{dh}{dt} = - \frac{dR}{dH}.$$

In these equations, according to the notation of Delaunay, $L = \sqrt{a \mu}$, μ being the sum of the masses of the earth and moon, $G = L \sqrt{1 - e^2}$, $H = G \cos i$; a, e, and i being the semi-major axis, eccentricity, and inclination respectively; l designates the mean anomaly, g the angular distance of the ascending node from the perigee, and h the longitude of the ascending node.

The equations which Le Verrier uses in his theories of the planets are not as simple in form as those of Delaunay; but there is no difficulty attending their derivation by this method. The method Le Verrier uses in deriving them is long and cumbrous. Delaunay does not stop to derive the equations he uses, but refers, on this head, to a memoir by Benôt.

By the method given above I have derived all the fundamental equations used by these authors, and by those who have considered the subject of perturbations from the same standpoint.

I think I have here given enough of the process to enable any one to understand the method. I may add that the method occurred to me seven or eight years ago.

The next communication was by Mr. EDGAR FRISBY

ON THE ORBIT OF SWIFT'S COMET.

This comet was first observed by Prof. Swift of Rochester, October 10, 1880, and was reported by him as moving directly towards the earth. It was observed by Prof. Eastman with the transit circle of the U. S. Naval Observatory on the evenings of October 25, November 7, and November 20, and from the data so obtained the following elements were computed by Prof. Frisby:

Epoch of perihelion passage $7^d.775675$ Washington mean time

$$\left.\begin{array}{rr} \Omega = & 296^\circ\ 48'\ 19.''9 \\ \pi = & 42^\circ\ 59'\ 15.''8 \\ \vartheta = & 42^\circ\ 26'\ 48.''5 \\ \iota = & 5^\circ\ 30'\ 35.''9 \\ \log a = & 0.517002 \\ \log \mu = & 2.774504 \end{array}\right\}\text{Mean Equinox 1880.0.}$$

From these elements it will be inferred that it was moving very nearly towards the earth at the time of discovery, October 10. On November 8, it came very near the earth's orbit, its distance from it then being about 0.069 of the earth's mean distance from the sun. The aphelion lies just beyond Jupiter's orbit so that its perturbations are liable at any time to become immense. The periodic time from the elements is about 2,178 days, or a little less than six years, but Jupiter's position in his orbit is now such that it is not likely to come near the comet for a long period. For a time after the discovery of the comet it was doubtful whether the period was 11 or 5½ years. The latter is undoubtedly the true one, the slight discrepancy being due to insufficient data. It would probably be impossible to see it at every return, for assuming its period to be approximately 5½ years, the earth would at each alternate return be at the opposite side of its orbit, and the sun would then intervene between the earth and the comet. It passed nearest to the earth about the 18th of November.

The logarithms of the radii vectors and distance from the earth on the dates given are:

	log. r	log. Δ
October 25,	0.085328	9.221510
November 7,	0.029018	9.141693
November 20,	0.034557	9.119295

No theory about any periodic time was assumed in these calculations.

At the conclusion of Mr. Frisby's paper the Society adjourned.

192D MEETING. JANUARY 22, 1881.

The President in the Chair.

Thirty-seven members present.

The following communication was read by Mr. J. W. CHICKERING, entitled—

NOTES ON ROAN MOUNTAIN, NORTH CAROLINA.

The great Appalachian chain, with its undulating line of 1,300 miles, from the promontory of Gaspè, on the Gulf of St. Lawrence, to Georgia and Alabama, beginning as a series of simple folds of moderate height, increases in complexity as in altitude from north to south, attaining its greatest elevation in a veritable mountain knot in the Black range. Following it from its commencement to the Hudson, we find the single chain of the Green Mountains, rising to its extreme height in Mount Mansfield, 4,430 feet, with, on the east, the outlying clusters of the White Mountains in New Hampshire, with Mount Washington reaching 6,288 feet, and others exceeding 5,000 feet, and Mount Katahdin in Maine, 100 miles away, about 5,200 feet, and on the west the Adirondack group, rising to 5,379 feet, and the Catskills considerably lower.

From the Hudson to the New River in Virginia, 450 miles, through the States of New Jersey, Pennsylvania, and Virginia, it gradually gains in both width and altitude, consisting of many parallel ranges, with fertile valleys between, of which the great valley of Virginia is the largest and best known. In Pennsylvania the summits vary from 800 to 2,500 feet. Toward the south the chains become more numerous and in Virginia the Peaks of Otter reach 4,000 feet. The extreme eastern range is called the Blue Ridge, the extreme western the Cumberland Mountains, or, more properly, Plateaus, while the high range or ranges between are, in general, called the Alleghanies.

From the New River south the system becomes much more complex. The main chain, hitherto called the Blue Ridge, is deflected to the west, and for 250 to 300 miles, in a circuitous chain, under the names of Iron, Stone, Bald, Great Smoky, and Unaka Mountains, forms the boundary line between North Carolina and Tennessee, rising frequently to heights exceeding 6,000 feet; while the more easterly range, retaining the name of Blue Ridge, and finding its southern terminus at Cæsar's Head, in South Carolina, where it turns abruptly to the northwest, reaches even loftier altitudes, Mitchell's high peak being accredited with 6,717 feet.

In North Carolina these two ranges are more than 50 miles apart, are partially connected by transverse ranges, and, for more than 100 miles, constitute a great central plateau, like that of Colorado on a small scale.

As says Prof. Guyot, "Here then through an extent of more than 150 miles the mean height of the valley from which the mountains rise is more than 2,000 feet. The mountains which reach 6,000 feet are counted by scores, and the loftiest peaks exceed 6,700 feet, while at the north, in the group of the White Mountains, the base is scarcely 1,000 feet, the gaps 2,000 feet, and Mount Washington, the only one which rises above 6,000 feet, is still 400 feet below the Black Dome of the Black Mountains. Here then, in all respects, is the culminating region of the vast Appalachian system."

The eastern chain, or Blue Ridge is still the watershed, and, on the Atlantic slope, gives birth to the Roanoke, Catawba, Broad, Saluda, and Savannah rivers; while on the other side this area of mountains and plateaus is separated by transverse chains into many deep basins, at the bottom of each one of which runs one of those mountain streams, which are compelled to cut their way to the Tennessee through gaps, gorges, and defiles in the very heart of this mighty chain, giving us some of the most picturesque scenery to be found on the continent. Among these, the New, Watauga, Nolichucky, and French Broad are the best known.

In the midst of this region, with all three ranges in sight, stands Roan Mountain, Laurentian in age, the State line crossing it at an altitude of 6,391 feet, as determined by the mean of my barometrical observations—and on and about this mountain it was my good fortune to stay from June 25th to August 30th.

Notes upon some of the peculiarities of the region, as contrasted with the northern Appalachian, will be my apology for asking your attention.

I. The Uniformity of Elevation.

Standing on the summit of Roan, we look into seven different States, and command a horizon of 30 to 80 miles. On the north and west the eye catches the Cumberland range in the horizon, beyond the great Tennessee plateau,. which is traversed by the Clinch and a score of other ranges, but all as level as if designed for railroad embankments.

On the south and east there is a wilderness of mountains. Guyot gives 50 to 60 with altitudes exceeding 6,000 feet, and yet the highest is only 6,717 feet, and perhaps 40 of them fall between 6,000 and 6,500, while hundreds of others are above 5,000. The valleys rarely go below 3,000 feet. The railroad after leaving Lynchburg reaches 1,000 feet in a few miles, and from that point for nearly 300 miles never goes below 1,500 feet, its highest summit being at 2,550 feet.

II. Uniformity of Temperature.

During nine weeks the mercury once indicated 75°, seven times 70° +, once 45°, three times 50°, the general daily variation being between 55° and 65°. The spring, a few rods rods from the hotel, has a temperature of 45°. Equally remarkable was the uniformity of atmospheric pressure the highest barometer being 24.19, and the lowest 23.87, or a difference of only 0.32 inches. No wind had a velocity of more than twenty miles an hour, and seldom did it reach ten.

III. Fertility of the Summit.

Instead of the upper 1,000 feet being, as in most of the northern Appalachian peaks reaching an altitude of over 5,000 feet, a pile of barren rocks, with lichens their only vegetation, the summit of Roan, and many other peaks, is a smooth, grassy slope, of the most vivid green, dotted with clumps of *Alnus viridis*, and *Rhododendron catawbiense*, the soil one or two feet in depth, rich and black. How this amount of humus was accumulated on these summits, and what cause destroyed the forests which its existence would seem to indicate as formerly existing, are questions not easily answered.

The valleys are very fertile, and adapted to almost any crop.

At an elevation of 3,000 to 4,000 feet occurs a belt of the most magnificent forest trees I have ever seen—hundreds of chestnuts, sugar maples, lindens, tulip trees, yellow birches, buck-eyes—some from 4 to 7 feet in diameter, and rising 70 to 80 feet without a limb. One chestnut measured 24 feet in circumference, and one black cherry measured 19 feet. Thorn bushes are as large as old apple trees with dwarf buck-eyes and yellow birches, looked like old orchards of vast extent.

IV. Flora.

Ascending the mountain, the vegetation takes on a northern aspect. Hemlocks abound till near the summit, where they are replaced by *Abies Fraseri*, the characteristic species of these summits.

Anemone nemorosa, Oxalis acetosella, Rubus odoratus, Ribes lacustre and *prostratum, Aster acuminatus. Habenaria articulata, Veratrum viride, Lycopodium lucidulum*, and similar species, remind one of the woods of Maine or New Hampshire.

The peculiar flora of the upper 1,000 feet, greatly resembles in habit that of the White Mountains, but very few species are the same. *Paronychia argyrocoma, Lycopodium selago* and *Alnus viridis*, are almost the only plants that occur to me as identical in the two localities, and these in the White Mountains are found in Crawford Notch, while in Roan they are near the summit. *Arenaria grœnlandica* is replaced by *A. glabra, Solidago thyrsoidea* by *S. glomerata; Geum radiatum* of the North is a variety of that found here; the two dwarf *Nabali* of White Mountains are represented by a new species, *N. roanensis, Rhododendron lapponicum* (four inches high) by magnificent *R. catawbiense*, covering the summit with its domes of inflorescence six to eight feet in diameter, *Castilleia pallida* by *C. coccinea*.

So that, in general, the species peculiar to these mountains are hardly sub-alpine, and thus continuous with similar species further north, but are rather apparent instances of local variation, many species being confined to very limited localities.

On Mount Washington, a few rods will often give the same plant in bud, flower, and fruit, as a north or south exposure, a precipice, or a snow-drift may retard or accelerate growth; but on these southern mountains no such difference obtains any more than in the valleys below.

On this communication Mr. J. W. POWELL remarked that the uniformity in the altitudes of the peaks is a feature resulting from the fact that the general mass out of which they have been carved by erosion possesses a plateau structure. The elevation of that region was distributed in its effects with an approach to uniformity over a wide extent of country, and was unaccompanied by those sharp flexings or the protrusions of abrupt mountain cores, which are encountered in some portions of the Appalachians and other mountainous regions. The individual masses and ranges in the Cumberland region are the work of erosion—the general process of land sculpture acting upon a broad platform, excavating broad valleys and narrow gorges, and leaving the peaks and ridges as cameos—mere remnants left in the general degradation of the whole region. Prof. Powell exemplified the process by citing the Uinta Mountains as a broad platform similarly carved by an extensive erosion.

The following paper was read by LESTER F. WARD, entitled—

FIELD AND CLOSET NOTES ON THE FLORA OF WASHINGTON AND VICINITY.

[Abstract.*]

Introductory Remarks.

This paper has resulted from a suggestion made to the writer in the spring of 1880, by a member of the Committee on Publications of this Society, relative to the need that exists for some special

*Mr. Ward's communication presented to the Society only a brief notice of the principal points of a monograph which he had prepared upon the flora of the District of Columbia. In view of the local character of his subject, and of the thorough and commendable manner in which it had been elaborated, the Committee on Communications recommended, and the General Committee authorized, the printing of a very full and copious abstract of the paper, which is given herewith.

treatise on the flora of this vicinity, and for a new and revised catalogue of the plants. While there now exists a provisional catalogue containing most of the species which have been collected or observed by botanists during the past six or seven years, it consists of so many small annual accretions, due to constant new discoveries, and contains withal so many blemishes and imperfections, incident to its hasty compilation and irregular growth, that it has ceased, in great part, to meet the demands of the present time. The elaboration of a systematic catalogue of the local flora was not, however, at the outset at all contemplated, but merely the presentation of certain notes and special observations on particular species, which had been made in the course of some nine years of pretty close attention to the vegetation, and somewhat varied and exhaustive field studies in this locality.

The flowering-time of most species here is much earlier than that given in the manuals, and is, moreover, in many cases, very peculiar and anomalous, rendering it important to collectors as well as interesting to botanists to have it definitely stated for a large proportion of the plants. It being thus necessary to extend the enumeration so far, it was thought that the remainder might as well be added, thus rendering it a complete catalogue of all the vascular plants known to occur here at the present time. To these has been appended the list of *musci* and *hepaticæ* prepared by the late Mr. Rudolph Oldberg for the *Flora Columbiana*, which has been left unchanged except in so far as was required to make it conform strictly to Sullivant's work which has long been the standard for this country. Dr. E. Foreman has also furnished the names of a few of the *Characeæ* collected by himself, and named by Prof. Farlow, of Cambridge, which, in the present unsettled state of the classification of the cryptogams, have, for convenience, been placed at the foot of the series.

In undertaking this compilation I have endeavored to resist the usual temptation of catalogue makers to expand their lists beyond the proportions which are strictly warranted by the concrete facts as revealed by specimens actually collected or species authentically observed; but have been content to set down only such as I can either personally vouch for, or as are vouched for by others who have something more substantial than memory to rely upon; preferring that a few species actually occuring but not yet seen should be omitted and afterwards supplied, rather than that others, supposed to exist, but which cannot be found, should stand in the catalogue to be apologized for to those who would be glad to obtain them. A few species, however, which are positively known to have once occured within our limits, but which have been obliterated within the recollection of persons now living, have been retained, as well as several of which only a single specimen has been found; but in all such cases the facts are fully stated in the notes accompanying each plant.

Range of the Local Flora.

The extent of territory which has of late years been tacitly recognized by botanists here as constituting the area of what has been called the *Flora Columbiana* is limited on the north by the Great Falls of the Potomac, and on the south by the Mount Vernon estate in Virginia, and Marshall's just opposite this on the Maryland side of the river, while it may reach back from the river as far as the divide to the east, and as far westward as the foot of the Blue Ridge, so as not to embrace any of the peculiarly mountain forms. Practically, however, the east and west range is much more restricted and only extends a few miles in either direction.

Comparison of the Flora of 1830 *with that of* 1880.

Washington and its vicinity has long been a field of botanical research. The year 1825 witnessed the dissolution of the *Washington Botanical Society*, which had for many years cultivated the science, and the same year also saw the formation of the *Botanic Club*, which continued the work, and in one respect, at least, excelled the former in usefulness, since it has handed down to us of the present generation a valuable record in the form of a catalogue of the plants then known to exist in this locality. This catalogue, which was fittingly entitled *Floræ Columbianæ Prodromus*, and claimed to exhibit "a list of all the plants which have as yet been collected," though now rare, and long out of print, is still to be found in a few botanical libraries.

I have succeeded in securing a copy of this work, and have been deeply interested in comparing the results then reached with those which we are now able to present. A few of these comparisons are well worth reproducing.

It should be premised that the *Prodromus* is arranged on the artificial system of Linnaeus, so that before the plants could be placed in juxtaposition they required to be re-arranged. This, however, was not the principal difficulty. Such extensive changes have taken place in the names of plants during the fifty years which have elapsed since that work appeared, (1830,) that it is only with the greatest difficulty that they can be identified. After much labor, I have succeeded in identifying the greater part of them, and in thus ascertaining about to what extent the two lists are in unison. This also reveals the extent to which each overlaps the other, and thus affords a sort of rude index to the changes which our flora has undergone in half a century. There are, however, as will be seen, many qualifying considerations which greatly influence these conclusions and diminish the value of the data compared.

The whole number of distinct names (species and varieties) enumerated in the *Prodromus* is 919. Of these 59 are mere synonyms or duplicate names for the same plant, leaving 860 distinct plants. I have succeeded in identifying 708 of these with certainty as among those now found, and six others, not yet clearly identified, should probably be placed in this class. This leaves 146 enumerated in the old catalogue which have not been found in recent investigations. [A classified list of these plants was presented and commented upon somewhat in detail.]

With regard to these 146 species, it must not be hastily concluded that they represent the disappearance from our flora of that number of plants. While they doubtless indicate such a movement to a certain extent, there are ample evidences that many of them can be accounted for in other ways. After careful consideration, I have been able to divide them into four principal classes arising out of—

1st. Errors on the part of those early botanists in assigning to them the wrong names.

2d. The introduction into the catalogue of adventitious and even of mere cultivated species, never belonging to the flora of the place.

3d. The undue extension by those collectors of the range of the local flora so as to make it embrace a portion of the maritime vegetation of the Lower Potomac or the Chesapeake Bay, and also the mountain flora of the Blue Ridge.

4th. The actual extermination and disappearance of indigenous plants during the fifty years that have intervened since they made their researches.

The assignment which I have made of each species to its appropriate class has been of course in great part conjectural and may be incorrect in many cases, while another botanist might have differed considerably in regard to special plants; yet it is not based on a general judgment drawn from my acquaintance with the present flora, but upon several kinds of special evidence, which in numerous instances has reversed my *prima facie* decision.

In the first place, I have carefully compared the range of each species as given in the text books to determine the probabilities for or against its being found here, and in the second place I have compared this list with the corresponding one of the species now found but not enumerated in the *Prodromus*. I have also endeavored to make due allowance on the one hand for the tendency above referred to to swell catalogues beyond their proper limits, and on the other for the well known fact that every flora is at all times undergoing changes.

It must not be forgotten, either, that half a century ago the surface of the entire country here must have presented a very different appearance from that which it presents now. The population of the District of Columbia in 1830, when it included a portion of Virginia, was only 39,834. It is now, exclusive of the Virginian part receded to that State, 177,638. To render the comparison more exact we may add to the latter number the present population of Alexandria county, amounting to 17,545, and we have in the place of 39,834 a population on substantially the same area of 195,183, or about five times as large. The population of Maryland in 1830 was 447,040; in 1880 it was 934,632, or considerably more than twice as large. That of Virginia in 1830 was 1,211,405. Virginia and West Virginia, embracing the same territory, now number 2,131,249 the population not having quite doubled: the retardation, however, as compared with Maryland, is doubtless due entirely to influences affecting the southern counties. There were doubtless large areas of primeval forest then within our limits which are now under cultivation, and a much greater variety of soil and woodland was then open to the researches of the botanist. As a consequence we ought to expect that it would sustain a much richer flora.

The general result at which I arrive by the process adopted may be summed up as follows:

1st. That 43 of these names, or 29 per cent. of them, belong to the first class and constitute errors in naming.

2d. That 12 of these plants, or 8 per cent., belong to the second class, or were simply cultivated species, and never belonged to this flora.

3d. That 10 of them, or 7 per cent., belong to the third class and were collected beyond the reasonable limits of our local flora.

4th. The remaining 81, or 56 per cent., belong to the fourth class, and represent *bona fide* discoveries in 1830 of species which either do not now occur or are so rare as to have escaped the investigations of the present generation of botanists.

With regard to the first of these classes, the large number of errors in naming cannot be considered any derogation from the ability or fidelity of the compilers of the *Prodromus* or their immediate predecessors, when we remember the very unsettled state that American botany was in at that time. Both names and authorities were badly confused, and errors were committed even by the most experienced botanists. For example, their *Corydalis glauca* as probably also their *C. aurea*, meant *C. flavula* which is now abundant, but omitted by them. Their *Arabis stricta* might have been *A. hirsuta* or *A. patens*, which are both now rare, though it was more probably a form of *A. laevigata*, as they seemed to be specially fond of drawing nice distinctions and expressing them by synonyms. Varieties, however, were scarcely recognized by them, the trinomial theory being then in its infancy. I might thus proceed to discuss all their supposed errors, but this is not necessary.

The second and third classes, amounting together to 16 per cent. of the alleged excess over the present flora, consist also of errors, but errors which it is much less easy to palliate. It is natural to wish to make as large a showing as possible, and the temptation to insert into a catalogue everything which by any construction can be claimed to belong there is rarely resisted. To show that this propensity still exists, it may be remarked that of the 1054 species enumerated in the preliminary catalogue of plants of this vicinity, published by the Potomac Side Naturalist's Club in 1876, 89, or about 8½ per cent. are now admitted by all not to have been seen here at that time, and have never been found by any one since, although nearly three hundred other species have since been added to the flora. This is certainly not a scientific method to proceed upon, and as already remarked, the present effort aims to eliminate to a great extent this source of error.

The 81 species constituting the fourth class remain, therefore, the only ones to which any special interest attaches and for the determination of which the present somewhat laborious analysis of this ancient document has been undertaken. For these, the botanists of our times should make diligent search and perchance a few of them may still be found. Assuming that they no longer exist, they do not represent the whole number of plants that have disappeared from our flora during the interval of fifty years. This could be only on the assumption that the *Prodromus* was a complete record of the flora at the time. This it certainly is not. The aggregate number, exclusive of synonyms or duplicated names, which it contained was, as we saw, 860, which includes one cellular plant, viz: *Achara*. We now identify, counting as was then done, species and varieties, 1249 distinct vascular plants. While no doubt many of these have been freshly appearing while others have been disappearing, still, from the considerations above set forth, it is highly probable that the indigenous flora of 1830 was considerably larger than that of 1880, and may have reached 1400 or 1500 vascular plants. It would appear, therefore, that only a little over half the plants actually existing were discovered by the botanists of that day, and enumerated in their catalogue. If the proportion of disappearances could be assumed to be the same for species not discovered as for those discovered by them, this would raise the aggregate number to considerably above one hundred, and perhaps to one hundred and twenty-five.

The great number of present known species not enumerated in the *Prodromus*, some of them among our commonest plants and amounting in the aggregate to 535 species, is another point of interest, since, after due allowance has been made for mistakes in naming them, it remains clear on the one hand that these researches must have been, compared with recent ones, very superficial; and on the other, that, not to speak of fresh introductions, many plants now common must have then been very rare, otherwise they would have proved too obtrusive to be thus overlooked.

Localities of Special Interest to the Botanist.

The flora of a wild region is always more uniform than that of one long subjected to human influences. The diversity in the former is a natural consequence of the corresponding diversity in the surface and other physical features. In the latter it is due to conditions arbitrarily imposed by man. A primeval flora is usually more rich in indigenous species, but the artificial changes caused by cultivation often offset this to a great extent by the introduction of foreign ones. This, however, greatly reduces its botanical interest.

In many respects the botanist looks at the world from a point of view precisely the reverse of that of other people. Rich fields of corn are to him waste lands; cities are his abhorrence, and great areas under high cultivation he calls "poor country;" while on the other hand the impenetrable forest delights his gaze, the rocky cliff charms him, thin-soiled barrens, boggy fens, and unreclaimable swamps and morasses are for him the finest lands in a State. He takes no delight in the "march of civilization;" the ax and the plow are to him symbols of barbarism, and the reclaiming of waste lands and opening up of his favorite haunts to cultivation he instinctively denounces as acts of vandalism. In him more than in any other class of mankind the poet's injunction—

"Woodman, spare that tree,"

touches a responsive cord. While all this may seem as absurd to some as does the withholding from tillage of great pleasure grounds in the form of hunting parks for the landed sporting gentry of Northern and Western Europe, still, when these parts of the world are compared with the artificially made deserts of Southeastern Europe and Western Asia, caused by the absence of such sentiments, there may, perhaps, be dimly recognized a "soul of good in things evil," if not a soul of wisdom in things ridiculous.

After the protracted subjection of a country to the conditions of civilization it gradually comes about that while the greater part of the surface falls under cultivation, more or less thorough, and the botanist is ultimately excluded from it, there will remain a few favored spots, which, from one cause or another, will escape and continue to form his favorite haunts. In the vicinity of large rivers, giving greater variety to the surface, or of rugged hills or mountains, this will be especially the case. As a country grows old large estates in the vicinity of cities fall into the possession of heirs who are engaged in mercantile or professional business, and neglect them, or they come into litigation lasting for years, and are thus happily abandoned to nature. These and other causes have operated in an especial manner in the surroundings of Washington, and there thus exist a large number of these green oases, as it were, interspersed over the otherwise botanical desert.

In consequence of this fact it requires experience in order to improve the facilities which the place affords. A botanist unacquainted with the proper localities for successful collection might spend a month almost in vain, and depart with the conviction that there was nothing here to be found. It may not be wholly peculiar, but these favored localities are here often of very limited extent, and in situations which from a distance afford no attraction to the collector. Civilization is, however, very perceptibly encroaching upon many of them, and it is feared that in another half century little will be left but a few bare rocks or inaccessible marshes.

In naming localities the principal authorities relied upon are: 1. A recent *Atlas of fifteen miles around Washington, including the County of Montgomery, Md., Compiled, Drawn, and Published from Actual Surveys, by G. M. Hopkins, C. E:* Philadelphia, 1879; and, 2, a military map of Northeastern Virginia, published in the work of General J. G. Barnard, on the *Defences of Washington*, 1821.

From the former the names of many roads, streams, estates, &c., have been obtained, while from the latter those of forts, batteries, &c., are often employed as more convenient. In this respect, however, much remains to be desired. While the military map is antiquated, the other is frequently defective in omitting what is required and incorrect in erroneously locating streams and other objects well known to the writer. In his extensive rambles he has learned many local names not found on the map, and in a few cases of special botanical interest, where names are wholly wanting, he has long been in the habit of designating the localities by names of his own christening, and for which he offers no apology.

The following are a few of the principal places of botanical interest which will be found to recur most frequently in the notes, and for this reason brief descriptions of them are appended.

1. *The Rock Creek Region.*—Rock Creek which forms the boundary line between Washington and Georgetown (West Washington), has escaped to a remarkable degree the inroads of agriculture and population. For the greater part of its length within the District of Columbia its banks are still finely wooded for some distance back, and afford a rich and varied field for botanical exploration. The character of the surface along Rock Creek is most beautiful and picturesque, often rocky and hilly with frequent deep ravines

coming down into the usually narrow bottom through which the creek flows. The stream itself is full of the most charming curves and the whole region is an ideal park. No one can see it without thinking how admirably it is adapted for a National Park. Such a park might be made to extend from Oak Hill Cemetery to the Military Road opposite Brightwood, having a width of a mile or a mile and a half. Not only every botanist but every lover of Art and Nature must sigh at the prospect, now not far distant, of beholding this region devastated by the ax and the plow. The citizens of Washington should speedily unite and strenuously urge upon Congress the importance of early rescuing this ready-made National Park from such on unfortunate fate.*

The Rock Creek Region is divided, so far as the designation of localities is concerned, into six sections. The first embracing the series of groves from Georgetown to Woodley Park on the right bank of the creek, is called Woodley. This section embraces several interesting ravines and in it are found many plants rare elsewhere, such as *Chamæ lirium, Carolinianum, Cypripedium pubescens, Hesperis matronalis* and *Liparis Lœselii.* In it is also a grove of the Hercules club (*Aralia spinosa.*) On the left bank of the creek lie the Kalorama Heights and some open woodland.

The *Woodley Park* section extends to the ravine which comes down opposite the old brick mill-ruin known as the Adams Mill. The timber here has been thinned out recently by the proprietors but not cleared off, and the vegetation has undergone a marked change. Several interesting plants have been found in Woodley Park, including the rare *Obolaria Virginica,* and the beautiful *Spiræa aruncus.* Above this the timber is heaviest on the left bank and some very fine ravines occur, at the head of one of which is a magnolia and sphagnum swamp where *Veratrum viride* and *Symplocarpus fœtidus* keep company with *Gonolibus obliquus* and *Pyrus*

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* It is remarkable that when committees of Congress have been appointed, as has several times been done, to consider a site for a National Park, they have usually looked in other directions and have seemed to ignore the existence of this region, which is certainly the only one that possesses any natural claims. A mere carriage ride through such parts as are traversed by roads is wholly insufficient to afford an adequate idea of its merits from this point of view. For the greater part of the distance mentioned above this region is accessible only to footmen.

arbutifolia. Here, too, though well up towards the ford, has been found *Polemonium reptans*, not seen elsewhere.

This third section terminates at Piney Branch, and from here to Pierce's mill, and as far above as the mouth of Brood Branch, the fourth section extends. This section is well wooded on both sides and includes the enchanting Cascade run which leaps down over the most romantic rocks. Near Pierce's mill are many trees and shrubs, planted there years before, but now well naturalized. Among these are *Aralia spinosa*, *Xanthoxylum Americanum*, *Acer saccharinum*, *Pinus strobus*, and *Carya alba*. Below the mill on the creek bottom is a long-abandoned nursery of *Populus alba* and *Acer dasycarpum*, from which many of the trees of the city may have been supplied.

From Broad branch to the Military road is the fifth and perhaps most interesting section of the Rock Creek Region. On the left bank lie the once noted Crystal Springs, and though the buildings are removed, the springs remain unchanged. Here have been found *Ophioglossum vulgatum*, *Anychia dichotoma*, and *Perilla ocimoides*, as well as *Tipularia discolor*. On the right bank and above Blagden's mill is a bold bluff in a short bend of the creek forming a sort of promontory upon which there grows *Gaultheria procumbens*, the winter-green or checkerberry, this being its only known locality within our limits. Half a mile farther up and back upon the wooded slope is the spot on which stand a dozen or more fine trees of the Table Mountain Pine, (*P. pungens.*) Here also was first found *Pycnanthenum Torreyi.*

To these there must be added a sixth section extending from the Brightwood road to the north corner of the District of Columbia which lies near Rock Creek. For the first mile there is little of interest, the cultivated land approaching the creek and the low hills near its banks being covered with a short second growth of scrub pine and black-jack. But above the Claggett estate on the right bank, and to some extent on both sides, lies the largest forest within our limits. This wood belongs, I learn, to the Carroll estate and is so designated in this catalogue. In it have been found very many most interesting plants. It was the first extensive tract found for the crowfoot (*Lycopodium complanatum*) and still constitutes the most reliable and abundant source known of this plant. Its present fame, however, rests upon its hybrid oaks, of which some most interesting forms have been found there. [See Field and Forest, October and November, 1875; Botanical Gazette, October, 1880, p. 123.] Here also grows very sparingly *Microstylis ophioglossoides*, and quite abundantly *Pyrola elliptica* and *P. secunda*. It is also a rich locality for many other species rare elsewhere.

2. *The Upper Potomac Region.*—The flora of the left bank of the Potomac is, in many respects, very unlike that of any other locality within our limits. A mile above Georgetown, and commencing from the recently constructed outlet lock of the Chesapeake and Ohio canal, there exists a broad and low strip of country formerly known by the name of Carberry Meadows, lying between the canal and the river, and extending to the feeder of the canal, a distance of about three and a half miles. This interval is relieved by two convenient landmarks, viz., one mile above the outlet lock, a grist-mill and guano factory, popularly known as Eads' mill; and a mile further, the celebrated Chain Bridge. Little Falls, proper, begin a hundred yards above the bridge, and extend half a mile or more. The region above the bridge will, therefore, be designated as Little Falls. The flats terminate in a remarkable knoll or small hillock of very regular outline and abrupt sides, which, from the combined effects of the feeder on one side, and large overflows from it below, becomes practically an island, and is well known to all as High Island. These river flats are, in most places, covered with large boulders of the characteristic gneiss rock of the country. In some parts the surface is very rough, and numerous pools or small ponds of water occur. Overflows and leakages from the canal cause large sloughs and quagmires, while annual ice-gorges crush down the aspiring fruticose vegetation. All these circumstances lend variety to the locality, and, as might be expected, the flora partakes largely of this characteristic. It would prolong this sketch unduly to enumerate all the rare and interesting plants which this region has contributed to our vegetable treasures, but conspicuous among them are *Polygonum amphibium*, var. *terrestre*, *Isanthus cœruleus*, *Herpestis nigrescens*, *Brasenia peltata*, *Cyperus virens*, and *Nesœa verticillata*, all of which recur below Ead's mill; *Ammannia humilis*, a remarkable variety of *Salix nigra*, (*S. nigra* var. *Wardi*, *Bebb.*) *Salix cordata*, and *S. longifolia*; as also *Spiranthes latifolia*, and *Samolus valerandi* var. *Americanus*, *Vitis vulpina* and *Panicum pauciflorum*, which may be found between this point and the bridge, while at the Little Falls we are favored with *Paronychia dichotoma*, *Œnothera fruticosa*, var. *lineare*

(very distinct from the type) and *Ceonothus ovatus*: also *Ranunculus pusillus* and *Utricularia gibba*. But rich and varied as are these lower flats, they are excelled by High Island, the flora of which is by far the most exuberant of all within the knowledge of botanists. Here we find *Jeffersonia diphylla, Caulophyllum thalictroides, Erigenia bulbosa, Silene nivea, Valeriana pauciflora, Erythronium albidum, Iris cristata*, and a great number of others of our most highly prized plants, many of which are found nowhere else.

Above the feeder is a series of islands in the river lying for the most part near the Maryland shore, and to which the maps, so far as I can learn, assign no names. The first of these lies well out in the river, and has been made to form a part of the feeder-dam. It is low and frequently overflowed, and has not, as yet, furnished many rare plants, though here *Arabis dentata* and some others have been found. It has been designated *Feeder-dam Island*. The second is half or three-quarters of a mile above, lies higher, and is covered with a very dense and luxuriant herbaceous vegetation and fine trees, chiefly of Box Elder, *Negundo aceroides*, from which circumstance and the peculiar impression which the long gracefully pendent staminate flower of these trees produced on the occasion of its first discovery by a botanical party it received the name of *Box Elder Island*. The third island is a short distance above the last, has a more elevated central portion and a similar vegetation. Here was found, on our first visit, and also on subsequent ones, *Delphinium tricorne*, and for this contribution to the Flora Columbiana it was christened *Larkspur Island*. The fourth of these islands is, in many respects, similar to the two last described, and upon it stands the only indigenous specimen of *Acer saccharinum* yet found here. It has, therefore, been appropriately named *Sugar-maple Island*. *Erythronium albidum, Trillium sessile, Jeffersonia diphylla* and similar species abound on all these islands, while on the Larkspur Island, besides the *Delphinium*, has also been found *Phacelia Purshii*. The beauty of these natural flower-gardens in the months of April and May is unequaled in my experience. The light and rich alluvial soil causes the vegetation to shoot up with magic rapidity at the first genial rays of the vernal sun, and often the harbinger of spring, *Erigenia bulbosa*, true to its name, will greet the delighted rambler in late February or early March.

The opposite, or Virginia side of the Upper Potomac, consists entirely of bold bluffs, interrupted by deep ravines, often containing wild torrents and dashing cascades. Here the flora, though less rich and varied, is also characteristic and interesting, and embraces, among other rare things, *Rhododendron maximum*, *Iris cristata*, *Scutellaria saxatilis*, *Pycnanthemum Torreyi*, *Solidago rupestris* and *S. virga-aurea*, var. *humilis*. On the Maryland side and a mile above the uppermost point thus far mentioned, is the Cabin John run, which the botanist celebrates more for its walking fern (*Camptosorus rhizophyllus*) than for the world-renowned arch that spans it.

The next most prolific source of interesting plants is the region of the Great Falls. The collecting grounds begin a mile or more below at Broad Water. On both sides of the canal the country is excellent, rocky and wooded, with stagnant pools and sandy hillocks. On these rocks grow *Sedum telephoides* and near Sandy Landing are found *Vitis vulpina*, *Arabis patens*, *A. hirsuta* and *Triosteum angustifolium*. In the pools have been found *Carex decomposita*, *Potamogeton hybridus* and *P. pauciflorus*, while on a rocky headland a large "water-pocket" has yielded my only specimen of the white water lily (*Nymphæa odorata*). *Cratægus parvifolia*, *Rumex verticillatus* *Steironema lanceolatum*, and last but not least, *Nasturium lacustre*, have also rewarded my researches in this singular and rather weird region.

On the opposite side of the river the site of the ancient canal around the Falls has proved very fertile in botanical trophies. *Polygala ambigua* is found near the boat landing, while by climbing the cliffs below this point the native of more northern climes may gaze once more upon his familiar Hemlock Spruce, *Tsuga Canadensis*. Difficult Run, a mile farther down, though indeed difficult of approach, repays the effort with *Podostemon ceratophyllus*, *Smilacina stellata*, *Potamogeton Claytonii*, and numerous other herbal treasures.

3. *The Lower Potomac Region.*

Passing next to the lower Potomac, the localities of special interest are, 1. Custis Spring, opposite the Arlington estate, with the extensive marsh below, where *Sagittaria pusilla*, *Discopleura capillacea*, *Cyperus erythrorhizus*, and other rare species are alone known to grow. 2. The point and bay below Jackson City, known as Roach's run, where are found, among others, *Scrophularia nodosa*, *Tripsacum dactyloides* and *Pycnanthemum lanceolatum*. 3. Four Mile run, half way to Alexandria, not yet sufficiently explored, including the vicinity of Fort Scott to the northwest, where *Clematis ochroleuca* and *Asclepias quadrifolia* may be collected; and, 4. Hunting creek, a large estuary below Alexandria, including Cameron run, the stream which debouches into it, with its tributaries, Back Lick run and Holmes run, which unite to form it. Here have been found, at various points, *Clematis ochroleuca, Gonolobus hirsutus, Itea Virginica, Geranium columbinum, Micranthemum Nuttallii, Habenaria virescens, Quercus macrocarpa, Carex gracillima, Geum strictum, Galium asprellum*, and very many other rare plants.

On the left bank of the lower Potomac the chief locality of interest is a large wooded area below the Government Hospital for the Insane. This has proved a rich hunting ground for the botanist, and has yielded *Carex pallescens, Carex Woodii, Gonolobus hirsutus, Silene armeria, Parietaria Pennsylvanica, Myosotis arvensis, Scutellaria nervosa*, &c., &c. *Asplenium angustifolium* is known only at Marshall Hall, where it has been reported by Mr. O. M. Bryan, while opposite Fort Foote Mr. Zumbrock has found *Myriophyllum spicatum*, and opposite Alexandria Professor Comstock and Miss Willets have discovered *Plantago cordata*.

4. *The Terra Cotta Region.*

This embraces some low grounds and undulating barrens near the terra cotta works, at Terra Cotta Station, on the Metropolitan Branch of the Baltimore and Ohio railroad, three miles from the city, and also a small swamp a quarter of a mile beyond, and to the eastward. Here on the dry ground have been found *Onosmodium Virginianum, Lespedeza Stuvei, Clitoria Mariana*, and *Habenaria lacera*; and in the swamp *Aster œstivus, Solida stricta, Woodwardia Virginica, Asclepias rubra, Poterium Canadense*, and numerous other plants rare or absent in other localities.

5. *The Reform School Region.*

This locality is very limited in extent, but has proved one of the most fertile in botanical rarities. Its nucleus consists of a little swampy spot a short distance to the south of the National Reform School, in which is located a beautiful spring; but the woody tract of country surrounding this and stretching southward and eastward some distance has also proved very fruitful. In the different portions of this region have been discovered *Phlox maculata, Melanthium Virginicum, Bartonia tenella, Lespedeza Stuvei, Desmodium Marilandicum* and *D. cilare, Buchnera Americana, Fimbristylis capillaris, Quercus prinoides, Carex bullata,* and *Gentiana ochroleuca,* most of which do not occur at all elsewhere.

6. *The Holmead Swamp Region.*

Like the last, this locality is quite circumscribed in area, but like it, too, it is rich in interesting plants. It occupies a ravine leading to Piney Branch from the east at the point where the continuation of Fourteenth street crosses that stream. The road connecting the last named with the Rock Creek Church road, and which is called Spring street, follows this valley. The collecting grounds are on the south side of this road and in the springy meadow along the rill. The timber has long been cut off, but the boggy character of the ground has thus far protected it from cultivation. The pasturing of animals on it during a portion of the year has latterly become a serious detriment to the growth of plants. Mr. Holmead, who owns it and lives near by, has kindly permitted botanists to investigate it for their purposes. Here have been found *Ludwigia hirsuta, Drosera rotundifolia Asclepias rubra, Xyris flexuosa, Fuirena squarrosa, Rhinchospora alba, Coreopsis discoidea* and the beautiful *Calopogon pulchellus* the most showy of our orchids.

In addition to these specially fertile tracts there are many other localities of great interest where valuable accessions to our flora have been made, and which will be particularly designated under the names of these species. It will suffice here to mention a wet meadow between the National Driving Park and Bladensburg, where, in a very diminutive spot, *Sarracenia purpurea, Viola lanceolata,* and *Carex bullata,* the two first wholly unknown elsewhere, have been discovered; a marsh a mile from Bladensburg, near the millrace, where only the majestic *Stenanthium robustum* has been seen; a little swamp near the Sligo creek, between the Riggs and Blair roads, where the Hartford fern (*Lygodium palmatum*) grows sparingly; and another between Bladensburg and the Maryland Agricultural College, where *Solidago elliptica, Ascyrum stans,* and *Lycopodium complanatum,* var. *Sabinæfolium,* have been found. The

Eastern branch region is not specially rich in floral treasures, but on its banks and marshes some good things appear. *Habenaria virescens*, *Steironema laceolatum*, *Eleocharis quadrangulata*, *Scirpus fluviatilis*, *Ranunculus ambigens*, and *Salix Russelliana* are among these, though some of them are found elsewhere.

Flowering time of Plants.

It has already been remarked that most species flower at Washington much earlier than at points farther north or the dates given in the manuals. In consequence of this, a botanist unacquainted with this fact, and accustomed to those climates and to relying upon the books, would be likely to be behind the season throughout the year, and fail to get the greater part of the plants he desired. With all my efforts to make allowance for this fact, I have frequently been sorely disappointed and was at last driven to making a careful record, preserving and correcting it from year to year, of the *flowering time* of plants in this locality. The notes on this subject appended to nearly every species enumerated in the list embody the general results of these observations and may in the main be relied upon. The expressions used are not loose conjectures, but are in the nature of compilations from recorded data. In most cases an allowance of two weeks may be made for the difference in seasons though rarely more and often less. Certain plants, as for example, *Tipularia discolor*, flower at almost exactly the same time every year. Occasionally, however, one will vary a month or more in a quite unaccountable way. But any one who has watched the periodical changes of the general vegetation for a series of years and recorded his observations, will more and more realize the exactness even of these complex biological phenomena which depend so absolutely upon uniform astronomical events.

From this point of view the season which presents the greatest variation and also, for this and other reasons, the greatest interest is the spring. There are a few plants which may sometimes be found in flower here in January, such as *Stellaria media*, *Taraxacum dens-leonis or Acer dasycarpum* (collected Jan. 17, 1876, in the city) in favored places, but these will bloom at any time when a few days of mild weather with sunshine can come to revive them. There are, however, several strictly vernal species which bloom quite regularly in the latter part of February, such as *Symplocarpus fœ-*

tidus, *Chrysosplenium Americanum*, and often *Anemone hepatica*. The number regularly found in flower in March is quite large and in special years very large. It was of course impossible to make observations every day of any year, but taking a number of years my observations cover nearly every day of the spring season. As showing the number of these early vernal species and also how widely the seasons may differ, the following facts are presented:

In the year 1878 seventeen species had actually been seen in flower and noted up to March 24th. I did not go out again that year until April 7, when I enumerated forty-six additional species, making sixty-three in all up to that date. This was an exceptionally early season. The next spring, that of 1879, was a backward one, as is shown by the fact that while I had visited the same localities, and taken notes with equal care only thirty-three species had been seen in flower up to April 13th: twenty-nine species which had been seen in flower on April 7th, 1878, were not yet in flower in the same localities on April 13th, 1879. There appeared to be about three week's difference in these two seasons. The last season, 1880, was again an early one, though less so than 1878. It was, however, near enough to the average to render the facts observed of great value. The following are a few of them: On February 29th, seven species were seen in flower in the Rock Creek region. On April 4th, thirty were enumerated on the Virginia side of the Potomac, above the Aqueduct Bridge. On April 11th, eleven were seen in addition to those previously enumerated in the Eastern Branch region: and on the 18th of April, High Island was visited, and twenty-nine added to all previously recorded, three of which were then in fruit. The total to this date was therefore seventy species. This season I concluded was a week or ten days later than that of 1878, and as much earlier than that of 1879.*

*Since the above was written the present season (1881) has passed its vernal period. It has proved still more backward than 1879 and the latest spring thus far observed. On April 3d, I made my first excursion and visited the Virginia side of the Potomac above Rosslyn. Only 7 species were seen in flower including *Alnus serrulata* which doubtless can be obtained much earlier in ordinary years, but has been overlooked. Besides *Draba verna*, a January species, and *Anemone hepatica*, a February one, the only herbaceous flower found was *Sanguinaria Canadensis*. On April 10th, High Island was visited, but only 8 species could be added to the above 7, and several of these, as *Jeffersonia diphylla*, *Dicentra cucullaria*, *Saxifraga Virginiensis*, *Erythronium Americanum*, and *Stellaria pu-*

We may now inquire what some of these early plants are. The following have been observed in flower in February:

Chrysosplenium Americanum, February 17, 1878.
Anemone Hepatica, February 20, 1876.
Salix Babylonica, February 22, 1874.
Populus alba, February 22, 1874.
Draba verna, February 24, 1878.
Acer dasycarpum, February 24, 1878,
Stellaria media, February 29, 1880.
Cerastium viscosum, February 29, 1880.
Claytonia Virginica, February 29, 1880.
Acer rubrum, February 29, 1880.
Symplocarpus fœtidus, February 29, 1880.

To these should, perhaps, be added *Equisetum hyemale,* which was found February 17, 1878, near the receiving reservoir with the spikes well advanced, quite contrary to the books which make it fruit in summer.

In addition to the above, which may often also be seen later, the the following have been noted flowering in March:

Populus alba, March 3, 1874,
Viola pedata, March 5, 1876.
Houstonia cœrulea, March 5, 1876.
Obolaria Virginica, March 5, 1876.
Dentaria heterophylla, March 8, 1874.
Poa brevifolia, March 8, 1874.
Capsella Bursa-pastoris, March 10, 1878.
Lamium amplexicaule, March 10, 1878.
Lindera Benzoin, March 10, 1878.
Epigaea repens, March 15, 1874.
Ulmus fulva, March 15, 1874.
Luzula campestris, March 15, 1874.
Saxifraga Virginiensis, March 16, 1879.
Sanguinaria Canadensis, March 17, 1878.
Sisymbrium Thaliana, March 17, 1878.

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bera, were very sparingly out. Cold weather continued to the end of the third week in April, and on April 24th, when High Island was again visited and a thorough canvas made, only 22 additional plants could be found there, and the whole number seen to that date was 46. The conclusion was that up to that time the season was about three weeks later than that of 1880.

Salix tristis, March 17, 1877.
Populus grandidentata, March 21, 1880.
Corydalis flavula, March 22, 1874.
Thalictrum anemonoides, March 24, 1878.
Dentaria laciniata, March 24, 1878.
Antennaria plantaginifolia, March 24, 1878.
Erodium cicutarium, March 27, 1874.
Erigenia bulbosa, March 28, 1875.
Cardamine hirsuta, March 30, 1879.

It is about the first of April, especially in early years, that the vegetation seems to receive the greatest impetus. This is well shown by the following list of species seen in flower during the first week in April:

Ulmus Americana, April 1, 1873.
Jeffersonia diphylla, April 2, 1876.
Cardamine rhomboidea, April 2, 1876.
Stellaria pubera, April 2, 1876.
Thaspium aureum, April 2, 1876.
Euphorbia commutata, April 2, 1876.
Alnus serrulata, April 3, 1881.
Ranunculus abortivus, April 4, 1880.
Dicentra Cucullaria, April 4, 1880.
Arabis laevigata, April 4, 1880.
Viola tricolor. var. arvensis, April 4, 1880.
Vicia Caroliniana, April 4, 1880.
Amelanchier Canadensis, April 4, 1880.
Nepeta Glechoma, April 4, 1880.
Sassafras officinale, April 4, 1880.
Carpinus Americana, April 4, 1880.
Ostrya Virginica, April 4, 1880.
Erythroncum Americanum, April 4, 1880.
Barbarea vulgaris, April 5, 1874.
Pedicularis Canadensis, April 5, 1874.
Mertensia Virginica, April 5, 1874.
Ranunculus abortivus, var. micranthus, April 7, 1878.
Ranunculus repens, April 7, 1878.
Asimina triloba, April 7, 1878.
Caulophyllum thalictroides, April 7, 1878.
Arabis dentata, April 7, 1878.

Barbarea praecox, April 7, 1874.
Sisymbrium Alliaria, April 7, 1878.
Viola cucullata, April 7, 1878.
Viola striata, April 7, 1878.
Viola glabella, April 7, 1878.
Ionidium concolor, April 7, 1878.
Silene, Pennsylvanica, April 7, 1878.
Cerastium vulgatum, April 7, 1878.
Cerastium oblongifolium, April 7, 1878.
Geranium, maculatum, April 7, 1878.
Oxalis corniculata, April 7, 1878.
Cercis Canadensis, April 7, 1878.
Potentilla Canadensis, April 7, 1878.
Thaspium trifoliatum, April 7, 1878.
Cornus florida, April 7, 1878.
Chrysogonum, Virginianum, April 7, 1878.
Senecio aureus, April 7, 1878.
Fraxinus viridis, April 7, 1878.
Phlox divaricata, April 7, 1878.
Lithospermum arvense, April 7, 1878.
Betula nigra, April 7, 1878.
Populus monilifera, April 7, 1878.
Arisaema triphyllum, April 7, 1878.
Erythronium albidum, April 7, 1878.
Trillium sessile, April 7, 1878.

My special observations on the vernal flowering time of plants extend about two weeks later or to the end of the third week in April, after which the great number of plants in bloom, including the amentaceous trees, render it difficult to pursue the investigation, while at the same time the facts become less valuable. The results for the second and third weeks of April, always excluding all previously enumerated, are as follows:

Arabis lyrata, April 9, 1876.
Fraxinus pubescens, April 11, 1880.
Salix cordata, April 11, 1880.
Salix purpurea, April 11, 1880.
Vaccinium corymbosum, April 12, 1880.
Carex platyphylla, April 12, 1880.
Poa annua, April 12, 1874.

Thalictrum dioicum, April 14, 1876.
Rhus aromatica, April 14, 1878.
Phlox subulata, April 14, 1878.
Arabis patens, April 18, 1880.
Cardamine hirsuta, var sylvatica, April 18, 1880.
Negundo aceroides, April 18, 1880.
Erigeron bellidifolius, April 18, 1880.
Krigia Virginica, April 18, 1880.
Sisyrinchium Bermudiana, April 18, 1880.
Carex laxiflora, April 18, 1880.
Carex Emmonsii, April 18, 1880.
Melica mutica, April 18, 1880.
Anemone nemorosa, April 19, 1874.
Viola cucullata, var. cordata, April 19, 1874.
Dirca palustris, April 19, 1874.
Carex Pennsylvanica, April 19, 1874.
Lathyrus venosus, April 21, 1878.
Ribes rotundifolia, April 21, 1878.
Salix nigra, var. Wardi, April 21, 1878.

We thus see that a single collector has in the course of eight year's operations actually observed and noted eleven species in bloom in February, 24 more in March, 51 additional in the first week of April, and 26 others during the second and third weeks of April or 112 up to April 21.

It should be remarked that there is no doubt that if the same localities in which the large numbers were observed on April 2 1876, April 4, 1880, and April 7, 1878 had been visited in the last days of March of those years quite a number of these plants would have been found sufficiently advanced to demand a place in the lists, and thus the month of March would have been credited with so many here set down for the first week in April. Probably, all things considered, not less than fifty species in certain favored seasons either reach or pass by their flowering-time by the end of March.

In arranging the above lists the order of dates has of course taken precedence, but where several are enumerated under one date the natural order is followed.

It is scarcely necessary to suggest a caution to collectors against relying upon these dates in making collections. They represent the earliest observations and not the average. In most cases an allowance of at least one week should be made for the full blooming of all the individuals of any given species. In all cases, however, one or more individuals were actually seen in flower and sufficiently advanced for collection, otherwise no note was taken. The *Carices* of course had not advanced to developed perigynia, and many plants whose inflorescence is centrifugal or centripetal, or which develop fruit while retaining their flowers, should be looked for at a later stage.

Autumnal Flowering.

One of the most interesting peculiarities of the flora of this vicinity is that of the second-blooming of vernal species, which in most cases takes place quite late in the fall. [See *Field and Forest*, April–June, 1878, Vol. III, p. 172.] In addition to the seven species observed and published in 1878, I have noted more than as many others manifesting this habit, and it is probable that still others will yet be added. The following is a list of those thus far recorded with the dates at which they were observed and which may be compared with those of their regular vernal period:

Ranunculus abortivus, var. micranthus, November 28, 1875.
Cardamine hirsuta, October 3, 1880.
Viola pedata, var. bicolor, September 22, and December 8, 1878
Viola striata, September 10, 1876.
Fragaria Virginiana, September 22, 1878.
Rubus villosus, September 22, and October 27, 1878.
Lonicera Japonica, October 13, 1878.
Houstonia purpurea, October 13, 1878.
Houstonia purpurea, var. angustifolia, September 12, 1880.
Houstonia cærulea, September 7, 1879.
Vaccinium stamineum, October 13, 1878.
Rhododendron nudiflorum, October 13, 1878.
Sabbatia angularis, October 27, 1878.
Phlox divaricata, October 16, 1873.
Echium vulgare, October 8, 1880.
Veronica officinalis, October 8, 1873.
Agrostis scabra, November 12, 1876.

To this list of seventeen should perhaps be added *Stellaria pubera*, which instead of a vernal and autumnal period, has two vernal periods as described under that species in the systematic notes.

Salix longifolia has this year (1881,) flowered twice; once in April and again in June.

Autumnal blooming, in so far as it is peculiar to this climate, may be chiefly attributed to the tolerably regular occurrence here of a hot and dry season in midsummer. This usually begins towards the end of June and ends about the middle of August. During this period, in some seasons, the ground and vegetation become parched and dried up, so that vegetal processes in many plants cease almost as completely as in the opposite season of cold. From this dormant state, the warm and often copious rains of the latter part of August revive them, as do the showers of spring, and they begin anew their regular course of changes. The frosts of October usually cut their career short before maturity is reached, but in some cases two crops of seed are produced. In addition to this, there frequently also occurs a very warm term in November, often extending far into December, and of this certain species take advantage and push forth their buds and flowers.

Albinos.

Well defined albinos have been collected of the following species

Desmodium nudiflorum.
Liatris graminifolia.
Rhododendron nudiflorum.
Vinca minor.
Mertensia Virginica.
Sabbatia angularis.
Pontederia cordata.

The green flowered variety of *Trillium sessile* is also common, and *Gonolobus obliquus* exhibits on High Island this same anomalous feature. *Carex tentaculata* having the spikes perfectly white, as if etiolated, was found June 14 of this year, (1881,) on the Eastern Branch marsh. This last phenomenon was certainly due neither to maturity or disease, but was a mere *lusus naturæ.*

Double Flowers, &c.

Thalictrum anemonoides, Ranunculus bullosus, Claytonia Virginica, and *Rubrus Canadensis,* have been found with the flowers much doubled as in cultivation.

Hydrangea arborescens occasionally has the outer circle of petals expanded as in cultivation.

Rudbeckia fulgida has been found with all its rays tubular but of the usual length.

Statistical View of the Flora.

In order to present a clear view of the general character of the vegetation of the District of Columbia and the adjacent country, I have made a somewhat careful analysis of the large groups and families, and comparison of them not only with each other, but with the same groups and families in larger areas and other local floras. The general results are presented below.

It is important to remark that in all enumerations, it is not simply the number of *species*, as at present recognized, but the number of *different plants*, (species and varieties,) that is employed. The reason for doing this is that in very many cases, well marked varieties are eventually made species, and if two plants really differ there is little probability that they will ever be merged into one species without that difference being indicated by some difference of name. The aim has therefore been to take account of the number of plants without regard to the manner in which they are named.

The whole number of vascular plants now known to this flora, as catalogued in the list appended to this paper, is 1249, and these belong to 527 different genera, or about 2½ species to each genus. These are distributed among the several systematic series, classes, and divisions, as follows:

Groups.	Genera.	Species and varieties.
Polyptelæ	174	356
Gamopetalæ	169	389
Total Dichlamydeæ	343	745
Monochlamydeæ (Apetalæ)	47	124
Total Dicotyledons	390	869
Monocotyledons	112	331
Gymnospermæ (Coniferæ)	4	7
Total Phænogamia	506	1,207
Cryptogamia	21	42
Total vascular plants	527	1,249

The percentages of the total are as follows:

Polypetalæ	33	29
Gamopetalæ	32	31
Total Dichlamydeæ	65	60
Monochlamydeæ (Apetalæ)	9	10
Total Dicotyledons	74	70
Monocotyledons	21	26
Gymnospermæ (Coniferæ)	1	1
Total Phænogamia	96	97
Cryptogamia	4	3

Large Orders.

The sixteen largest orders arranged according to the number of species, are as follows:

	Genera.	Species and varieties.
1. Compositæ	53	149
2. Gramineæ	43	110
3. Cyperaceæ	10	108
4. Leguminosæ	24	57
5. Rosaceæ	15	46
6. Labiatæ	23	42
7. Cruciferæ	16	33
8. Scrophulariaceæ	15	32
9. Filices	16	30
10. Ranunculaceæ	7	27
11. Ericaceæ	11	26
12. Cupuliferæ	7	26
13. Orchidaceæ	12	24
14. Liliaceæ	18	24
15. Polygonaceæ	3	23
16. Umbelliferæ	17	22

The whole number of systematic orders represented in our District is 116, of which sixteen, or 14 per cent. furnish 55 per cent. of the genera and 62 per cent. of the species.

Large Genera.

The fifteen large genera arranged according to the number of plants are the following:

		Species and varieties.
1.	Carex	70
2.	Aster	21
3.	Panicum	19
4.	Solidago	18
5.	Quercus	18
6.	Polygonum	16
7.	Desmodium	14
8.	Salix	14
9.	Juncus	14
10.	Viola	13
11.	Cyperus	12
12.	Ranunculus	11
13.	Eupatorium	11
14.	Helianthus	10
15.	Asclepias	10

Thus fifteen, or less than three per cent., of the genera furnish 271, or nearly 22 per cent. of the species.

Introduced Species.

The whole number of introduced plants enumerated in the subjoined catalogue is 193, of which 15 are supposed or known to be indigenous to other parts of the United States.* These are distributed through the several larger groups as follows:

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* These are the following:

Xanthoxylum Americanum.
Trifolium repens.
Prunus Chicasa.
Rosa setigera.
Philadelphus inodorus.
Ribes rotundifolium.
Ribes rubrum.
Passiflora incarnata.
Symphoricarpus racemosus.
Symphoricarpus vulgaris.
Catalpa bignonioides.
Maclura aurantiaca.
Populus grandidentata.
Poa annua.
Pinus Strobus.

	Old World.	United States.	Total.
Polypetalous	65	8	73
Gamopetalous	54	3	57
Apetalous	28	2	30
Monocotyledonous	31	1	32
Coniferæ	--	1	1
Total	178	15	193

It will be seen that the introduced plants amount to 15.5 per cent. of the total flora.

The several orders to which these belong, are shown in the summary.

Shrubby Species.

Of the 342 "Forest Trees" enumerated in Sargent's preliminary catalogue of 1880, this flora embraces 85, or 24.8 per cent., of which 65 are large enough to have the dignity of timber trees. Of these 85, 25 are in the Polypetalous Division, but only 12 of this latter number are large; 9 are in the Monopetalous Division, all but 2 of which are large; 44 are in the Apetalous Division, 39 of which are large; and the remaining 7 are Coniferous, all full-sized trees.

The whole number of species which are shrubby or woody above ground is 194, which is 15.5 per cent. of the whole; they are distributed as follows:

Polypetalous	83
Gamopetalous	36
Apetalous (Monochlamydeous)	64
Monocotyledonous (Endogenous)	4
Gymnospermous (Coniferous)	7
Total	194

For further particulars the reader can consult the Summary at the end of the catalogue.

Comparisons with other Floras.

While these facts are of great interest in affording a clear conception of the character of our flora, they do not aid us in determining in what respects it is peculiar or marks a departure from those of other portions of the country, or from that of the country at large. To institute comparisons with other local floras would of course carry me much too far for the general purpose of this paper, but it is both more interesting and more practicable to confront a few of the above results with similar ones, drawn from a consideration of a large part of the United States. For this purpose, as not only most convenient but as least liable to embrace facts calculated to vitiate the comparisons, I have chosen that portion of the United States situated east of the Mississippi river, and for the most part well covered by *Gray's Manual of Botany* for the Northern portion and *Chapman's Flora of the Southern States* for the Southern. The plants described in these works are conveniently collected into one series by the second edition of *Mann's Catalogue*, published under the supervision of the authorities at Cambridge, in 1872. Many changes have since been made in the names, &c., and a few new species added, but these are not sufficient to affect the general conclusions to be drawn from the following comparative tables.

Comparison of Species and Varieties.

The number of species and varieties of vascular plants enumerated in the work above referred to is 4,034, of which the 1,249 of the flora of Washington, by groups, is as follows:

	Species and varieties in the		Per Cent.
	Eastern U. S.	Flora Columbiana.	
Polypetalæ	1,115	356	32
Gamopetalæ	1,314	389	30
Total Dichlamydeæ	2,429	745	31
Monochlamydeæ (Apetalæ)	349	124	36
Total Dicotyledons	2,778	869	31
Monocotyledons (Endogens)	1,034	331	32
Gymnospermæ	28	7	25
Total Phænogamia	3,840	1,207	31
Cryptogamia	194	42	22
Total vascular plants	4,034	1,249	31

Comparison of Genera.

The whole number of genera in the flora of the Eastern United States is 1065. That of the Flora Columbiana, as already stated is 527. This is over 49 per cent., a much larger proportion than was shown by a comparison of the species. A comparison of the genera by classes, gives the following results:

	Genera represented in the		Per Cent.
	Eastern U. S.	Flora Columbiana.	
Polypetalæ	340	174	51
Gamopetalæ	379	169	45
Total Dichlamydeæ	719	343	48
Monochlamydeæ (Apetalæ)	97	47	48
Total Dicotyledons	816	390	48
Monocotyledons	198	112	57
Gymnospermæ	12	4	33
Total Phænogamia	1,026	506	49
Cryptogamia	39	21	54
Total vascular plants	1,065	527	49

The percentages here range from 33 in the Gymnosperms to 57 in the Monocotyledons, averaging between 49 and 50, whereas in the similar comparisons for species they ranged from 22 in the Cryptogams to 36 in the *Monochlamydeæ*. This result was to be expected since as the groups increase, the number represented in any local flora should be proportionally larger. For example, 116 orders out of the 156 are represented here, which is upwards of 74 per cent.

Comparison of Large Orders.

It will be interesting to compare in a manner similar to the foregoing, the number of species in several of the largest orders. For this purpose we may use the same orders mentioned a few pages back as the richest in species of any belonging to this flora. The comparison may then be shown as follows:

Orders.	Eastern U. S.	Flora Col.	Per Cent.
1. Compositæ	497	149	30
2. Gramineæ	297	110	37
3. Cyperaceæ	357	108	30
4. Leguminosæ	208	57	27
5. Rosaceæ	104	46	44
6. Labiatæ	121	42	35
7. Cruciferæ	76	33	43
8. Scrophulariaceæ	97	32	33
9. Filices	134	30	22
10. Ranunculaceæ	80	27	34
11. Ericaceæ	89	26	29
12. Cupuliferæ	45	26	58
13. Orchidaceæ	71	24	34
14. Liliaceæ	82	24	29
15. Polygonaceæ	56	23	41
16. Umbelliferæ	63	22	35

This table exhibits better perhaps than any other the special charateristics of the flora. The normal percentage being about 31, we see that in all but five of these sixteen largest orders our flora is in excess of that standard, while it is richest proportionally in the *Cupuliferæ*, *Rosaceæ*, and *Cruciferæ*, and poorest in the *Filices*, and *Leguminosæ*.

Comparison of Large Genera.

In like manner we may compare the fifteen large genera given in a preceding table.

Genera.	Eastern U. S.	Flora Col.	Per Cent.
1. Carex	180	70	39
2. Aster	63	21	33
3. Panicum	36	19	53
4. Solidago	61	18	30
5. Quercus	38	18	47
6. Polygonum	27	16	59
7. Desmodium	24	14	58
8. Salix	23	14	61
9. Juncus	38	14	37
10. Viola	24	13	54
11. Cyperus	41	12	29
12. Ranunculus	27	11	41
13. Eupatorium	24	11	46
14. Helianthus	27	10	37
15. Asclepias	22	10	45

This table shows that in all the large genera except Solidago and Cyperus, the District of Columbia has more than its full proportion. The genus *Salix* is the one proportionally best represented, while *Polygonum, Desmodium, Panicum* and *Viola,* each exceed 50 per cent. *Quercus, Eupatorium* and *Asclepias* are also well filled out.

As already remarked, it would carry us too far to undertake the systematic comparison of our flora with those of other special localities, even were the data at hand. Few local catalogues are condensed and summarized for this purpose and the labor of doing this is very great. The recently published *Flora of Essex County Massachusetts,* prepared by Mr. John Robinson, however, forms something of an exception to this, and we may directly compare the larger classes and also the orders. The following tables will give an idea of the differences between that flora and our own:

Series, Classes, and Divisions.	Number of Orders.		Number of Genera.		Number of Species and Varieties.	
	Essex County.	Washington.	Essex Connty.	Washington.	Essex County.	Washington.
Polypetalæ	42	45	155	174	360	356
Gamopetalæ	25	27	158	169	358	389
Total Dichlamydeæ	67	72	313	343	718	745
Monochlamydeæ	18	19	44	47	132	124
Total Dicotyledons	85	91	357	390	850	869
Monocotyledons	17	20	120	112	392	331
Gymnospermæ (Coniferæ)	1	1	7	4	17	7
Total Phæenogamia	103	112	484	506	1,259	1,207
Cryptogamia	5	4	20	21	65	42
Total vascular plants	108	116	504	527	1,324	1,249

The sixteen large orders enumerated on page 89 may also be compared with profit:

Large Orders.	Number of Genera.		Number of Species and Varieties.	
	Essex County.	Washington.	Essex County.	Washington.
1. Compositæ	43	53	136	149
2. Gramineæ	50	43	128	110
3. Cyperaceæ	9	10	120	108
4. Leguminosæ	17	24	39	57
5. Rosaceæ	12	15	55	46
6. Labiatæ	22	23	35	42
7. Cruciferæ	14	16	29	33
8. Scrophulariaceæ	14	15	29	32
9. Filices	13	16	40	30
10. Ranunculaceæ	9	7	30	27
11. Ericaceæ	18	11	37	26
12. Cupuliferæ	6	7	16	26
13. Orchidaceæ	13	12	32	24
14. Liliaceæ	18	18	27	24
15. Polygonaceæ	3	3	27	23
16. Umbelliferæ	16	17	20	22

In the flora of Essex County, the orders *Umbelliferæ* (20) and *Cupuliferæ* (16) fall below the lowest of the sixteen for the flora of Washington, (*Umbelliferæ* 22,) while on the other hand the *Caryphyllaceæ* (27,) *Salicaceæ* (23,) and *Naiadaceæ* (28,) not in the list, rise above that number. These orders in the flora of Washington are represented respectively by 19, 19, and 9 species and varieties. With reference to the last named of these orders, however, it may be remarked that the genus *Potamogeton*, which constitutes the greater part of it, has been imperfectly studied here, and will certainly be largely increased when thoroughly known.

The orders in which this flora falls below that of Essex county are: the *Gramineæ*, *Cyperaceæ*, *Rosaceæ*, *Filices*, *Ranunculaceæ*, *Ericaceæ*, *Liliaceæ*, *Orchidaceæ*, and *Polygonaceæ*, nine in all. In the remaining seven orders there is a greater number of species here than there. It is noteworthy that our flora exceeds that of Essex county most in the *Compositæ*, *Leguminosæ*, and *Cupuliferæ*, and next to these in the *Scrophulariaceæ*, *Labiatæ* and *Cruciferæ*. Our comparatively poorest orders are the *Cyperaceæ*, *Rosaceæ*, *Ericaceæ* and *Filices*. Comparing in like manner the fifteen large genera enumerated on page 90 we are able to see still more definitely wherein the two floras differ.

Large Genera.	Number of Species and Varieties.	
	Essex County.	Washington.
1. Carex	71	70
2. Aster	25	21
3. Panicum	14	19
4. Solidago	19	18
5. Quercus	10	18
6. Polygonum	21	16
7. Desmodium	7	14
8. Salix	18	14
9. Juncus	14	14
10. Viola	11	13
11. Cyperus	11	12
12. Ranunculus	13	11
13. Eupatorium	7	11
14. Helianthus	5	10
15. Asclepias	7	10

The total number of species and varieties represented by these fifteen genera is thus considerably larger in the Washington flora (271,) than in that of Essex county, (253;) but whereas they are absolutely the largest genera here, this is not the case there. The genus *Potamogeton* numbers 23 in Mr. Robinson's Catalogue, and the genus *Scirpus* 14, while several others probably exceed ten. Those in the above list falling below ten, the lowest on the Washington list, are *Desmodium* (7,) *Eupatorium* (7,) *Asclepias* (7,) and *Helianthus* (5.) Those in which the Essex flora exceeds the Washington flora are *Carex*, *Aster*, *Solidago*, *Polygonum*, *Salix* and *Ranunculus*, though *Carex*, *Solidago* and *Cyperus* may be regarded as equal in the two floras, and *Juncus* is exactly equal. In *Quercus*, *Desmodium*, *Eupatorium*, *Helianthus* and *Asclepias*, the Essex flora is poor, only amounting in the second and fourth named, to half the number found here.

Relative to the above comparisons in general, it may be remarked first, that the flora of Essex county, Massachusetts, is much more thoroughly and exhaustively elaborated than that of the District of Columbia, lying as it does in the immediate center of botanical activity in this country. This alone is probably sufficient to account for all the difference in the number of species in the two localities, and it will probably be ultimately found that the two floras are very nearly equal. In the second place, if it should be thought that from its intermediate location between the southern and the northern sections of the country, our flora should naturally be the more rich in species, it may be satisfactorily urged on the other hand, that while we have only an inland territory, Essex county has both an inland and a maritime territory. Could our range be extended to embrace even a small extent of sea coast, the number would thereby be very largely increased.

As a final statistical exhibit, more comprehensive in its scope, and from a different point of view, I give below a table in which our local flora is compared not only with the floras above named, but with several others in America. As these several floras not only overlap to a considerable extent, but also differ widely in the total number of plants embraced by each, it is evident a numerical comparison would convey a very imperfect idea of the variety in their essential characteristics. It is therefore necessary to reduce them to a common standard of comparison, which has been done by disregarding the actual numbers and employing only the percentage which each group compared bears to the total for each respective flora. The relations of the several groups to the total vegetation of each flora is thus brought out, and a comparison of the percentages of the same group in the different areas displays in the clearest manner possible the predominance or scantiness of the groups in each flora. Upon this must depend, in so far as botanical statistics can indicate it, the *facies* of each flora, its peculiarities and characteristics. As in previous comparisons, the table is restricted to Phenogamous and vascular Cryptogamous plants, and the same groups are employed, except that the large genera are omitted, while the number of orders is increased to the 23 largest of this flora, which is taken as the basis of comparison, and they are arranged in the order of rank with reference to it.

The several floras compared with the total number of plants embraced in each, are as follows:

1.	Flora of Washington and vicinity	1,249
2.	Flora of Essex county, Massachusetts	1,324
3.	Flora of the State of Illinois	1,542
4.	Flora of Northeastern United States	2,365
5.	Flora of Southeastern United States	2,696
6.	Flora of Eastern United States (= 4 + 5)	4,034
7.	Plants collected by the Fortieth Parallel Survey	1,254
8.	Plants collected by Lieut. Wheeler's Survey	1,535

For the flora of Illinois, (No. 3,) and also for that of the Northern United States, east of the Mississippi, (No. 4,) I have used, without verification, the figures of the *Catalogue of the Plants of Illinois*, 1876, prepared by Mr. Harry N. Patterson, as summarized in the preface. In the former case, the introduced species are included, but the varieties seem to be excluded. In the latter case, as stated by Mr. Patterson, the introduced species are excluded, as are also doubtless the varieties,

For the flora of the Southern United States, east of Mississippi, (No. 5,) which I have compiled from Dr. Chapman's *Flora of the Southern States*, indigenous species are alone taken, in order to make it conform as nearly as possible to the flora of the Northeastern United States, (No. 4.)

The plants collected by the Fortieth Parallel Survey, (No. 7,) and those collected on Lieut. Wheeler's Survey, (No. 8,) are introduced rather as a means of contrasting the Eastern with the Western portions of the continent, than as a proper part of the comparative botanical statistics of this vicinity. The former of these collections was very thoroughly and carefully made by an energetic and experienced botanist, Mr. Sereno Watson, and derives its chief value from this fact. It embraces, however, a territory having a somewhat special character from a botanical point of view, viz: in general terms, the Great Basin between the Rocky Mountains and the Sierra Nevada, and the High Plateaus and mountains immediately adjacent, (Wasatch, Uintas, Sierras,) with a restricted range north and south. The data are taken from the summary of the work prepared by Mr. Watson, and found on page XIV of the Report. The collections embraced in the Report of Lieut. Wheeler's Survey, on the other hand, were made by numerous collectors, some of them amateurs, and were scattered over a very wide extent of western territory, including Colorado, New Mexico, Utah, Arizona and Nevada, and continued through five years of exploration. They may be taken therefore to represent, with some correctness, the general character of our Western Flora, exclusive of the Pacific Coast. The facts given are derived from the "Table of Orders" on page 379. In both cases varieties are excluded.

For the remaining floras compared in the table, (Nos. 1, 2, and 6,) to avoid re-compilation, the data previously used are repeated, species and varieties, including also introduced plants, being employed. As already intimated, however, this difference in the basis of compilation of different floras, applying as it does to the several groups and to the aggregate alike, cannot materially affect the percentages as computed.

The following is the Table of Percentages:

Groups.	Flora of Washington and Vicinity.	Flora of Essex County, Massachusetts.	Flora of the State of Illinois.	Flora of the Northeastern United States.	Flora of the Southeastern United States.	Flora of the total Eastern United States.	Plants collected by the 40th Parallel Survey.	Plants collected by Lieut. Wheeler's Survey.
Polypetalæ	28.5	27.2	28.5	26.8	28.9	27.6	35.1	31.9
Gamopetalæ	31.1	27.0	32.2	31.6	34.7	32.6	36.0	35.8
Total Dichlamydeæ	59.6	54.2	60.7	58.4	63.6	60.2	71.1	67.7
Monochlamydeæ	9.9	10.0	9.8	7.9	8.8	8.7	9.8	10.6
Total Dicotyledons	69.5	64.2	70.5	66.3	72.4	68.9	80.9	78.3
Monocotyledons	26.5	27.6	25.5	29.0	24.3	25.6	16.4	15.7
Gymnosperms	0.6	1.3	0.7	0.9	0.7	0.7	1.2	1.3
Total Phænogamia	96.6	95.1	96.7	96.2	97.4	95.2	98.5	95.3
Cryptogamia	3.4	4.9	3.3	3.8	2.6	4.8	1.5	4.7
Total vascular plants	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0

Orders.	Flora of Washington and Vicinity.	Flora of Essex County, Massachusetts.	Flora of the State of Illinois.	Flora of the Northern United States.	Flora of the Southern United States.	Flora of the total Eastern United States.	Plants collected by the 40th Parallel Survey.	Plants collected by Lieut. Wheeler's Survey.
1. Compositæ	11.9	10.3	13.0	12.2	13.7	12.3	16.5	16.6
2. Gramineæ	8.9	9.7	7.8	7.5	7.2	7.4	5.4	7.8
3. Cyperaceæ	8.6	9.1	8.5	10.5	8.0	8.9	4.4	3.8
4. Leguminosæ	4.6	2.9	4.7	4.3	6.1	5.2	7.2	8.2
5. Rosaceæ	3.7	4.2	3.2	3.0	2.2	2.6	3.4	2.9
6. Labiatæ	3.4	2.6	2.8	2.2	2.8	3.0	0.9	2.2
7. Cruciferæ	2.6	2.2	2.1	2.0	1.4	1.9	4.4	2.8
8. Scrophulariaceæ	2.6	2.2	2.7	2.3	2.5	2.4	4.5	4.8
9. Filices	2.4	3.0	2.3	2.4	2.1	3.3	1.0	4.3
10. Ranunculaceæ	2.2	2.3	2.7	2.3	1.9	2.0	3.0	2.3
11. Ericaceæ	2.1	2.8	0.9	2.9	2.0	2.2	1.3	0.9
12. Cupuliferæ *	2.1	1.8	1.4	1.5	1.3	1.4	0.4	0.9
13. Liliaceæ	1.9	2.0	2.1	2.4	2.1	2.0	3.0	1.5
14. Orchidaceæ	1.9	2.4	1.8	2.4	1.9	1.7	0.6	0.5
15. Polygonaceæ	1.8	2.0	1.9	1.1	1.5	1.4	4.0	3.2
16. Umbelliferæ	1.8	1.5	1.8	1.7	1.6	1.6	2.4	1.2
17. Caryophyllaceæ	1.5	2.0	1.4	1.5	1.5	1.5	2.2	1.6
18. Salicaceæ	1.5	1.7	1.2	0.8	0.3	0.7	0.9	0.8
19. Onagraceæ	0.9	1.1	1.2	1.2	1.3	1.1	2.3	2.4
20. Saxifragaceæ	0.7	1.0	0.8	1.5	0.9	1.1	2.1	1.4
21. Chenopodiaceæ	0.7	1.3	0.7	0.5	0.5	0.6	2.1	1.5
22. Naiadaceæ	0.7	2.1	1.2	1.2	0.4	1.0	0.7	0.3
23. Polemoniaceæ	0.5	0.1	0.5	0.3	0.5	0.4	3.3	1.8

* Including the Betulaceæ.

Comparisons have already been made of our local flora with that of Essex county, Massachusetts, which contains so nearly the same number of plants. In examining the percentages in the above table, these distinctions are equally manfest. In both divisions of the *Dichlamydeæ*, and also in the Dicotyledons, and the total *Phænogamia*, our flora is richer than that of Essex county, while in the *Monochlamydeæ*, the Monocotyledons, the Gymnosperms, and the Cryptogams, it falls below. In the *Compositæ*, *Leguminosæ*, *Labiatæ*, *Cruciferæ*, *Scrophulariceæ*, *Cupuliferæ*, and a few other orders it is in excess, while in the *Gramineæ*, *Cyperaceæ*, *Rosaceæ*, *Filices*, &c., the Essex flora leads.

In the comparison with the flora of the State of Illinois, one is struck by the marked similarity in the position of the groups, notwithstanding the well known differences in the actual species. In the *Gamopetalæ*, and total *Dichlamydeæ*, as also in the *Monochlamydeæ* the difference is very slight, while in the *Polypetalæ* it disappears entirely. The Dicotyledons are therefore nearly the same, and we find this true also of the Monocotyledons, and the Gymnosperms. Whatever slight variations occur in the above named groups, they are so adjusted as nearly to balance each other, so that when we reach the total *Phænogomia*, we again have substantial unison, which of course is maintained in the *Cryptogamia*.

This harmony is less pronounced in the larger orders, the *Compositæ* being richer, and the *Gramineæ* poorer there than here. In the *Cyperaceæ*, *Leguminosæ*, *Scrophulariaceæ*, and *Filices*, the difference is not great, but in the *Rosaceæ*, *Labiatæ*, *Cruciferæ*, and *Cupuliferæ*, the Washington flora is decidedly in advance, and in the *Ericaceæ* it is of course in very marked contrast. In the *Orchidaceæ*, *Polygonaceæ*, *Umbelliferæ*, *Caryophyllaceæ*, and *Polemoniaceæ*, there is substantial, or exact identity. In the *Ranunculaceæ*, *Onagraceæ*, *Naiadaceæ*, and *Liliaceæ*, besides the *Compositæ* already mentioned, the Illinois flora leads that of Washington. On the whole there is a remarkable similarity in the facies of these two floras, which may be due to their inland situation, with fluriatile areas, and similar position as to latitude. Considering, however, the marked specific peculiarities of the flora of the flat prairies of the West, we would have naturally looked for a corresponding distinctness in the larger groups and orders.

The comparisons of our flora, from this point of view, with those of the Northern and Southern States, east of the Mississippi river, and with these two combined, as represented in the next three columns, proves of the highest interest, and will repay somewhat close inspection. It has often been asked, to what extent the flora of Washington is affected by influences of a peculiarly southern character, and while it has generally been conceded that it belongs clearly to the northern section of the country, many facts, such as those previously set forth, relative to autumnal flowering and early flowering, as well as to the number of species, which exhibit more or less green foliage throughout the winter, combine to give it a decidedly southern aspect. In so far as the method of testing such questions which has been here adopted can be relied upon, this southern leaning on the part of the Washington flora is clearly exhibited in this table. In letting the eye follow columns four and five, the differences are well marked in nearly all the groups, and in most of the large orders. These are what express statistically the essential characteristics of the northern as contrasted with the southern flora. It is also obvious that the figures in column six will, in most cases, express the mean between these two extremes. To obtain the true position of our flora, it is necessary to observe toward which of these extremes it most nearly approaches, and whether it falls on the northern or southern side of the mean established by column six. In instituting this comparison, we perceive at the outset, that in the Polypetalous division, it falls so far on the southern side as to come within four tenths of one per cent. of being identical with the flora of the Southern States. In the *Gamopetalæ*, however, it agrees quite closely with the flora of Northern States, so that in the *Dichlamydeæ* as a whole, it coincides very well with the mean for both sections. The *Monochlamydeæ* agree better with those of the Southern States and the total Dicotyledons fall largely on the southern side of the mean. The Monocotyledons also fall somewhat on the southern side, while the Gymnosperms are below the mean which here corresponds with the southern flora. This leaves the total Phænogams, occupying an intermediate position. The Cryptogams are also very nearly intermediate, though approaching the northern side.

Considering next the relations of the large orders, we find that in the *Compositæ* our flora is northern in aspect. In the *Gramineæ* it is very exceptionally rich, surpassing all the larger areas and approaching that of Essex county, Massachusetts. In the *Cyperaceæ*, which are peculiarly typical for the purpose, on account of being indigenous in all the floras, it does not correspond at all, either with the northern section or with the average of both sections, but does agree very closely with the exceptionally meager representation of the southern flora. The *Leguminosæ* are here northern in aspect, the *Rosaceæ*, like the *Gramineæ*, exceptionally rich, far exceeding either section, as is also the case with the *Labiatæ* and the *Cruciferæ*. The ferns are northern in their degree of representation, as are the *Ranunculaceæ* while the *Ericaceæ* and *Scrophulariaceæ* are southern. The *Cupuliferæ* again are anomalous and tower above all other floras. The *Liliaceæ* are southern, as are also the *Orchidaceæ*. The *Polygonaceæ* are in excess, and in so far southern in aspect, while the *Umbelliferæ*, also in excess, denote a northern inclination. The *Caryophyllaceæ* are remarkable for showing the same percentage in all of the four floras now under comparison. The *Salicaceæ* are largely in excess of every flora compared in the table, except that of Essex county, Massachusetts, while *Onagraceæ* and *Saxifragaceæ* both fall below the normal, the latter, however, showing a southern tendency. The *Naiadaceæ* are southern, as are also the *Polemoniaceæ*, while the *Chenopodiaceæ* are slightly in excess in their degree of representation.

Now, as this locality has been classed as northern, we should not expect to find it occupying an intermediate position, which would place it on the boundary line between the northern and the southern flora, but we should expect to find it agreeing closely with the northern flora, or at least lying midway statistically, as it does geographically, between the dividing line or medium, represented by the total eastern flora and the northern flora. So far is this from being the case, however, that we actually find it occupying a position considerably below the medium line, and between this and the line of the southern flora; a position which would be geographically represented by the latitude of Nashville or Raleigh, or even by Memphis or Chattanooga.

This result is very remarkable, and while the proofs from statistics are, perhaps, not alone to be relied upon, it serves to confirm many facts recorded which have puzzled the observers of the phenomena of the vegetable kingdom in this locality.

The results of the careful comparison of the two remaining columns need not be here summed up, as the reader will readily perceive their general import, and he will not be likely to stop with considering the relations of the local flora with those of the far West, but will probably seek for more general laws governing the vegetation of the eastern and western sections, as we have already done to some extent for the northern and southern sections.

Abundant Species.

It was Humboldt who remarked that of the three great Kingdoms of Nature, the Mineral, the Vegetable, and the Animal, it is the Vegetable which contributes most to give character to a landscape. This is very true, and it is also true, that botanists rarely take account of this fact. The latter are always interested in the relative numbers of species belonging to different Classes, Families, and Genera, rather than to the mere superficial aspect of the vegetation. It is, however, not the number of species, but individuals which give any particular flora its distinguishing characteristics to all but systematic botanists, and it is upon this, that in the main depends the commercial and industrial value of the plant-life of every region of the globe. It is often the omnipresence of a few, or even of a single, abundant species that stamps its peculiar character upon the landscape of a locality. This is to a far greater extent true of many other regions, especially in the far West, than it is of this; the vegetation of the rural surroundings of Washington is of a highly varied character, as much so perhaps as that of any part of the United States. And yet there are comparatively few species, which from their abundance chiefly lend character to the landscape, and really constitute the great bulk of the vegetation. The most prominent, if not actually the most numerous of these, are of course, certain trees and notably several species of oak. Probably the most abundant tree here, as in nearly all parts of the country, is *Quercus alba*, the white oak; but *Q. prunus*, the chestnut oak, *Q. coccinea*, the scarlet oak, *Q. palustris*, the swamp oak, and *Q. falcata*, the Spanish oak, are exceedingly common. The most abundant hickory is *Carya tomentosa*, the mockernut. *Liriodendron tulipifera*, the tulip-tree, often improperly called white poplar, besides being one of the commonest trees, is the true monarch of our forests, often attaining immense size. It is a truly beautiful tree whose ample foliage well warrants the recent apparently successful experiments in introducing it as a shade tree for the streets of the city. Among other common trees may be mentioned the chestnut, (*Castanea vulgaris, Lam, var. Americana*, A. D. C., the beech, (*Fagus ferruginea*,) the red maple, (*Acer rubrum*,) the sycamore, (*Platanus occidentalis*,) the red or river birch, (*Betula nigra*,) the white elm, (*Ulmus Americana*,) the sour gum, (*Nyssa multiflora*,) the sweet gum, (*Liquid-amber Styraciflua*,) the scrub pine, (*Pinus inops*,) the pitch pine, (*P. rigida*,) and the yellow pine, (*P. mitis*.)

Of the smaller trees, *Cornus florida*, the flowering dogwood and *Cercis Canadensis*, the red-bud or Judas tree are very abundant, and chiefly conspicuous in the spring from the profusion of their showy blossoms; all three species of sumac are common. *Hamamelis Virginica*, the witch-hazel, and *Virburnum prunifolium* the black haw abound; *Sassafras officinale*, sassafras, *Castania pumila*, the chinquapin and *Juniperus Virginiana*, the red cedar also belong to this class.

Of the smaller shrubby vegetation, we may safely claim as abundant *Cornus sericea*, and *C. alternifolia*, the silky, and the alternate-leaved normal *Viburnum acerifolium*, *V. dentatum*, and *V. nudum*, arrow-woods, *Gaylussacia resinosa*, the high-bush huckleberry, *Vaccinium stamineum*, the deer berry, *V. vacillans* and *V. corymbosum* the blueberries, *Leucothoë racemosa*, *Andromeda Mariana*, the stagger bush, *Kalmia latifolia*, the American laurel, or calico-bush, *Rhododendron nudiflorum*, the purple azalea flower, *Lindera Benzoin*, the spice bush.

Of vines besides three species of grape which are abundant, we have *Ampelopsis Virginiana*, the Virginian creeper or American woodbine, *Rhus toxicodendron*, the poison ivy, and *Tecoma radicans*, the trumpet vine, which give great beauty and variety to the scenery.

The most richly represented herbaceous species may be enumerated somewhat in their systematic order. Of *Polypetalæ*, may be mentioned *Ranunculus repens*, *Cimicifuga racemosa*, *Dentaria laciniata*, *Viola cucullata*, *Viola pedata*, *var. bicolor*, and *V. tricolor*, *var. arvensis*; *Stellaria pubera*, *Cerastium oblongifolium*, *Geranium maculatum*, *Impatiens pallida*, and *I. fulva*; *Desmodium nudiflorum*, *D. acuminatum*, and *D. Dillenii*; *Vicia Caroliniana*, *Potentilla Canadensis*, *Geum album*, *Saxifraga Virginiensis*, *Oenothera fruticosa*, and *Thaspium barbinode*. In the *Gamopetalæ* before *Compositæ*, we have *Galium aparine*, *Mitchella repens*, *Houstonia purpurea*, and *H. cærulea*. In the *Compositæ*, the most conspicuous are; *Vernonia Noveboracense*, *Eupatorium purpureum*, *Liatris graminifolia*, *Aster patens*, *A. ericoides*, *A. simplex* and *A. miser*, *Solidago nemoralis*, *S. Canadensis*, *S. altissima*, and *S. ulmifolia*; *Chrysopsis Mariana*, *Ambrosia trifida*, and *A. artemisiæfolia*, (these behaving like introduced weeds;) *Helianthus divaricatus*, *Actinomeris squarrosa*, *Rudbeckia laciniata*, and *R. fulgida*; *Coreopsis verticillata*, *Bidens cernua*, *Verbesina Siegesbeckia*, *Gnaphalium polycephalum*, *Antennaria plantaginifolia*, *Hieracium venosum*, and *H. Gronovii*; *Nabalus albus*, and *N. Traseri*, *Lactuca Canadensis*.

The remaining *Gamopetalæ* furnish as abundant species: *Lobelia spicata*, *Chimaphila umbellata*, and *C. maculata*; *Veronica officinalis*, and *V. Virginica*, *Gerardia flava*, *Verbena hastata*, and *V. urticifolia*; *Pycnanthemum incanum*, and *P. linifolium*, *Collinsonia Canadensis*, *Salvia lyrata*, *Monarda fistulosa*, and *M. punctata*; *Nepeta glechoma*, *Brunella vulgaris*, *Mertensia Virginica*, *Flox paniculata*, and *P. divaricata*; *Solanum Carolinense*, and *Asclepias cornuti*.

Of herbaceous *Monochlamydeæ* may be named *Polygonum Virginianum*, *P. sagittatum*, and *P. dumetorum*; *Laportea Canadensis*, *Pilea pumila*, and *Bœmehria cylindrica*.

The *Monocotyledons* give us *Arisæma triphyllum*, the Indian turnip, *Sagittaria variabilis*, *Aplectrum hyemale*, *Erythronium Americanum*, *Luzula campestris*, *Juncus effusus*, *Juncus marginatus*, and *Juncus tenuis*, *Pontederia cordata*.

Of the *Cyperi*, *C. phymatodes*, *C. strigosus* and *C. ovularis* are the most common. *Eleocharis obtusa* and *E. palustris*; *Scirpus pungens*, *S. atrovirens*, *S. polyphyllus*, and *S. eriophorum*, are very conspicuous. Of *Carices*, *C. crinata*, *C. intumescens*, the various forms of *C. laxiflora*, *C. platyphylla*, *C. rosea*, *C. scoparia*, *C. squarrosa*, *C. straminea*, *C. stricta*, *C. tentaculata*, *C. virescens* and *C. vulpinoides*, are the most obtrusive. In the *Gramineæ*, those which most uniformly strike the eye are *Agrostis scabra*, *Muhlenbergia Mexicana*, and *M. sylvatica*, *Tricuspis seslerioides*, *Eatonia Pennsylvanica*, *Poa pratensis*, *Poa sylvestris*, and *P. brevifolia*,; *Eragrostis pectenacea*, *Festuca nutans*, *Bromus ciliatus*, *Elymus Virginicus*, *Danthonia spicata*, *Anthoxanthum odoratum*, *Panicum virgatum*, *P. latifolium*, *P. dichotomum*, (with a multitude of forms,) and *P. depauperatum*; *Andropogon Virginicus*, and *A. scoparius*.

Of ferns *Polypodium vulgare*, *Pteris aquilina*, *Adiantum pedatum*, *Asplenium ebeneum*, and *A. Filix-fœmina*; *Phegopteris hexagonoptera*, *Aspidium acrostichoides*, *A. marginale* and *A. Noveboracense*; *Osmunda regalis*, *O. Claytoniana*, and *O. cinnamonea*, are the most constantly met with.

Lycopodium lucidulum is quite common, and *L. complanatum* is very abundant in certain localities.

Besides the above, which are all indigenous to our flora, there are many introduced species in the vicinity of the city, and of cultivation everywhere which manifest here as elsewhere, their characteristic tendency to crowd out other plants and monopolize the soil.

Such are the most general features which the traveler accustomed to observe the vegetable characteristics of localities visited, may expect to see when he pays his respects to the Potomac valley. To some even this imperfect description might furnish a fair idea of our vegetable scenery without actually seeing it.

Classification Adopted.

In endeavoring to conform to the latest authoritative decisions relative to the most natural system of classification, I have followed, with one exception, the arrangement of the *Genera Plantarum* of Bentham and Hooker so far as this goes, and the accepted authorities of Europe and America for the remainder. For the *Gamopetalæ* after *Compositæ*, however, covered by Prof. Gray's *Synoptical Flora of North America*, I have followed that work which is substantially in harmony with the *Genera Plantarum*. In the arrangement of the orders, too, for the *Polypetalæ*, Mr. Sereno Watson's *Botanical Index* has in all cases been conformed to, as also not materially deviating from the order adopted by Bentham and Hooker. In the genera there are numerous discrepancies between the works last named, and in the majority of these cases the American authorities have been followed. For example, Bentham and Hooker have thrown *Dentaria* into *Cardamine*, *Elodes* into *Hypericum*, and *Ampelopsis* into *Vitis*, and *Pastinaca* and *Archemora* into *Peucedanum*. The change of *Spergularia* to *Lepigonum* is adopted, as well as a few alterations in orthography where the etymology seemed to demand them, as Pyrus to Pirus and Zanthoxylum to Xanthoxylum. I have also declined to follow Bentham and Hooker in the changes which they have made in the terminations of many ordinal names. The termination *aceæ* is doubtless quite arbitrary in many cases, and, perhaps, cannot be defended on etymological grounds but as a strictly ordinal ending it has done good service in placing botanical nomenclature on a more scientific footing. It is also true that the old system does not always employ it, as in some of the largest orders, *e. g. Cruciferæ, Leguminosæ, Compositæ, Labiatæ;* but whatever changes are made should rather be in the direction of making it universal than less general. Bentham and Hooker do not adopt a universal termination, neither do they abolish the prevailing one, and they retain it in the majority of cases; but in certain cases, for which they doubtless have special reasons, they substitute a different one, and one which is often far less euphonious. The following are the orders represented in this catalogue in which the termination *aceæ* is retained by American and altered by English authorities.

American.	*English.*
Berberidaceæ.	Berberideæ.
Cistaceæ.	Cistineæ.
Violaceæ.	Violarieæ.
Polygalaceæ.	Polygaleæ.
Caryophyllaceæ.	Caryophylleæ.
Portulacaceæ.	Portulaceæ.
Hypericaceæ.	Hypericineæ.
Celastraceæ.	Celastrineæ.
Vitaceæ.	Ampelideæ.
Saxifragaceæ.	Saxifrageæ.
Hamamelaceæ.	Hamamelideæ.
Lythraceæ.	Lythrarieæ.
Onagraceæ.	Onagrariæ.
Passifloraceæ.	Passifloreæ.
Cactaceæ.	Casteæ.
Valerianaceæ.	Valerianeæ.
Asclepiadaceæ.	Asclepiadeæ.
Gentianaceæ.	Gentianeæ.
Borraginaceæ.	Borragineæ.
Scrophulariaceæ.	Scrophularineæ.
Lentibulaceæ.	Lentibulariceæ.
Plantaginaceæ.	Plantagineæ.
Nyctaginaceæ.	Nyctagineæ.
Lauraceæ.	Laurineæ.
Juglandaceæ.	Juglandeæ.
Salicaceæ.	Salicineæ.
Ceratophyllaceæ.	Ceratophylleæ.

On the other hand, the British authorities are followed in uniting the *Saururaceæ* with the *Piperaceæ*, and also in placing the *Paronychieæ*, reduced to a sub-order under the *Illecebraceæ;* but from the certain relationship of this order with the *Caryophyllaceæ*, it is deemed unnatural to separate these two orders by putting the former into the *Monochlamydeous* division. [See American Naturalist, November, 1878, p. 726.] On the same ground of apparently close relationship, I have followed Bentham and Hooker in abolishing the *Callitrichaceæ*, and placing *Callitriche* in the *Halorageæ*. On the other hand I have followed Gray in retaining the *Lobeliaceæ*, as also in keeping the *Ericaceæ* intact, and not slicing off the *Vacciniaceæ* from one end, and the *Monotropeæ* from the other, as is done in the *Genera Plantarum*.

In the *Gamopetalæ*, before and including *Compositæ*, in the *Monochlamydeæ*, and throughout the *Monocotyledons*, serious difficulties occur in consequence of a want of recent systematic works from the American point of view. In nearly all cases the names as well as the arrangement of Gray's Manual, 5th edition, have here been adopted. I have, however, been able to avail myself of a number of recent revisions of genera made by Gray, Watson, and Engelman* and published in various forms, chiefly in the Proceedings of the American Academy of Arts and Sciences. I have also derived many useful hints from the *Flora of California*, from the botanical reports of the various Western Surveys, from Sargent's Catalogue of the Forest Trees of North America, and from the Flora of Essex county, Massachusetts.

Mr. M. S. Bebb, of Rockford, Illinois, has shown great kindness not only in determining all the uncertain *Salices*, but in generously drawing up a list of them in the order of their nearest natural relationship, which is followed implicitly in the catalogue.

For the Ferns, the magnificent work of Prof. Eaton has furnished everything that could be desired, and is unswervingly adhered to.

The following genera in the *Compositæ* have been changed by Bentham and Hooker, but the new names cannot be adopted until the species have been worked up by American botanists. The old ones are therefore retained with a simple indication of the recent disposition.

Maruta has been made Anthemis.
Leucanthemum has been made Chrysanthemum.
Cacalia has been made Senecio.
Lappa has been made Arctium.
Cynthia has been made Krigia.
Mulgedium has been made Lactuca.
Nabalus has been made Prenanthes.

* While I have gladly adopted the arrangement of the species of *Quercus* decided upon by Dr. Engelman after so careful a study, I cannot do so without recording a gentle protest against the position to which he assigns *Q. palustris*. viz: between *Q. falcata*, and *Q. nigra*, and far removed from *Q. rubra*. Not only the shallow, finely scaled cup, but especially its light colored buds and thin early leaves, as also a special *facies* belonging to its aments and foliage ally this species with *Q. rubra*, and distinguish these two species as a group from all others found in this flora.

Several of these cases are a return to the older names, and whether they will be adopted by American authorities it is impossible to say.

It remains to consider the one deviation above referred to from the prevailing system of botanical classification, which it has been thought proper to make in the subjoined list of plants. This consists in placing the *Gymnosperms*, here represented only by the single order *Coniferæ*, after the *Monocotyledons* and next to the *Cryptogams*.

It is not the proper place here to state the already well known grounds upon which this position of the Gymnosperms has been defended. [See American Naturalist, June, 1878, pp. 359 to 378.] It is sufficient to point out that the correctness of this arrangement was recognized by Adrien de Jussieu, and has been repeatedly maintained by later botanists of eminence. The object in adopting it here, however, is not simply because it seems fully justified by the present known characters of plants, for consistently to do this would also require that the *Polypetalæ* be placed before the *Monochlamydeæ* (in the descending series,) and that numerous other changes be made. So wide a departure from the existing system would seriously detract from the convenience of the work as a practical aid to the local botanist, and aside from the labyrinth of nice and critical points into which it must inevitably lead, it would not be advisable in the present state of botanical literature. But as the position of the *Gymnosperms* is the most glaringly inconsistent of all the defects of the present so-called Natural System, and as the *Coniferæ* are represented here by only four genera and seven species, it is evident that no serious objection could arise on the ground of inconvenience, while at the same time it may serve some useful purpose in directing the minds of botanists who may look over the work to the obvious rationality of this classification, and contribute its mite towards awakening them to the recognition of a truth which, I cannot doubt, must sooner or later find expression in all accepted versions of the true order of nature with respect to the vegetable kingdom.

Common Names.

I am well aware that in recent times it has become more and more the practice among botanists to eschew all common or popular names of plants. This sentiment I share to a great extent and will therefore remark at the outset that the best common name for a plant is always its systematic name, and this should be made a substitute for other popular names wherever and whenever it can be done. In most cases the names of the genera can be employed with entire convenience and safety; and in many cases they are to be defended on the ground of euphony. How much better, for example, the name *Brunella* sounds than either Self-heal, or Heal-all, both of which latter, so far as their meaning goes, express an utter falsehood. Some works professing to give common names frequently repeat the generic name, as such. This has seemed to me both unnecessary and calculated to mislead. It is not done where other accepted common names exist, and thus the implication is that in such cases it is incorrect to use the Latin name. Again it is only done for the commoner species, leaving it to be inferred that there is no popular way of designating the rarer ones. The plan here followed is to regard the genus as the best name to use in all cases, and as *ex officio* the proper common name of every plant, and, therefore, not in need of being repeated in different type as such in any case. But in addition it has been deemed best to give such appropriate or well established common names as can be found. Some scientific men seem disposed to forget that it is the things rather than the names that constitute the objects of scientific study. There is a vast amount of true scientific observation made by mere school-girls and rustics, who do not know the name of the branch of science they are pursuing. A knowledge of a plant by whatever name or by no name at all is scientific knowledge, and the devotees of science should care less for the means than the end which they have in view. Individuals differ in their constitution and character. The sound or sight of a Latin word is sometimes sufficient, in consequence of ineradicable, constitutional or acquired idiosyncrasies, to repel a promising young man, or woman, from the pursuit of a science for which genuine aptitude and fondness exist. For such and other classes, common English names have a true scientific value. The object should be to inspire a love for plants in all who can be made to take an interest in them, and to this end to render the science of Botany attractive by every legitimate means available. In so far, therefore, as English names of plants can be made conducive to this end, they should be employed. Their inadequacy to the true needs of the science in its later stages cannot fail to impress itself upon all who pursue it to any considerable extent.

Finally common names are not wholly without their scientific uses. A few of them have proved more persistent than any of the systematic names, as I have had occasion to observe in examining the *Prodromus Floræ Columbianæ* of 1838, in which difficult work, I must confess, they frequently rendered me efficient aid in determining the identity of plants, which the Latin names used did not reveal.

In appending common names to the plants of this vicinity *The Native Wild Flowers and Ferns of the United States*, by Prof. Thomas Meehan, has been followed in most cases, so far as this work goes, but this of course embraces but a fraction of the entire flora. Most of the remaining names are taken from Gray's Manual of Botany, and from his Synoptical Flora of the United States. In many cases some of the names given which do not seem appropriate are omitted, and in a few cases those given have been slightly changed. A small number of local names given, not found in any book, but in themselves very expressive, have been given, as "curly head" for *Clematis ochroleuca*, &c.; and in a few other cases, names have been assigned to abundant species on the analogy of those given for allied genera or species.

Concluding Remarks.

The foregoing remarks on the value of common names naturally suggest a few general reflections with which our introduction will conclude.

The popularization of science is now a leading theme of scientific men. To accomplish this, certain branches of science must first become a part of liberal culture. The pursuit of fashion, which is usually regarded as productive solely of evil, may be made an agency of good. If it could become as much of a disgrace to be found ignorant of the flora or fauna of one's native place as it now is to be found ignorant of the rules of etiquette or the contents of the last new növel, devotees of Botany and natural history would immediately become legion, and the woods and fields would be incessantly scoured for specimens and objects of scientific interest. It should be the acknowledged work of educationalists to make science fashionable and call to their aid these powerful social sentiments in demanding the recognition of its legitimate claims.

Of all the natural sciences, that of Botany is the most easily converted into a branch of culture. Its objects appeal directly to the highest esthetic faculties. It naturally allies itself with the arts of drawing, painting, and sketching, and the deeper the insight into its mysteries the stronger does it appeal to the imagination. Its pursuit, besides being the best possible restorer of lost, and preserver of good health, is a perpetual source of the purest and liveliest pleasure. The companionship of plants, which those who do not know them cannot have, is scarcely second to that of human friends. The botanist is never alone. Wherever he goes he is surrounded by these interesting companions. A source of pure delight even where they are familiarly known to him, unlike those of his own kind, they grow in interest as their acquaintance grows less intimate, and in all his travels they multiply immensely his resources of enjoyment.

The man of science wonders what the unscientific can find to render travel a pleasure, and it must be confessed that a great many tourists of both sexes go at the behest of fashion, and care little more for nature when crossing the Alps than did Julius Cæsar, who could only complain of the bad roads and while away the hours in writing his grammatical treatise, *De Analogia.* While all forms of natural science, so far from paralyzing the esthetic faculties, tend powerfully to quicken them, that of Natural History and especially of Botany awakens such an interest in Nature and her beautiful objects, that those who have once tasted pleasure of this class may well consider other pleasures insipid.

But notwithstanding these attractions which Botany possesses above other sciences, there exists among a small class of scientific men a disposition to look down upon it as lacking scientific dignity, as mere pastime for school-girls or fanatical specialists. This feeling is most obvious among zoölogists, some of whom affect to disdain the more humble forms of life and the simplicity of the tame and stationary plant.

This sentiment, though now happily rare, is natural and really constitutes what there is left of that proud spirit with which man has ever approached the problems of Nature. His first studies disdained even so complicated an organism as man himself, and spent themselves in the pursuit of spiritual entities wholly beyond the sphere of science. Later he deigned to study *mind* detached from body and from matter, still later he attacked some of the higher manifestations of *life*. Ethics came next, and social organizations; then anthropological questions were opened, and next those of physiology and anatomy, and at last comparative anatomy and structural zoölogy. Phytology brought up the rear and was long confined to the most superficial aspects. It is only in recent times that plants and all the other lowly organisms have begun to receive proper attention, and only since this has been done has there been made any real progress in solving the problem of Biology.

It is a paradox in science that its most complicated forms must first be studied and its simplest forms last, while only through an acquaintance with the latter can a fundamental knowledge be obtained. The history of biological science furnishes many striking illustrations of this truth, the most interesting of which is perhaps to be found in the labors of the two great French savants, Cuvier and Lamarck. The former spent his life and powers in the study of vertebrate zoölogy amid the most complex living organisms. The latter devoted his energies to Botany and to Invertebrate Zoölogy, including the protozoan and protistan kingdoms. The former founded his great theory of types, and his cosmology of successive annihilation and reconstructions of the life of the globe. The latter promulgated his theory of unbroken descent with modification. The conclusions of the former were accepted in his day, and are rejected in ours, those of the latter were rejected in his own lifetime, but now form the very warp of scientific opinion.

Let no botanist, therefore, or person contemplating the study of Botany be deterred by the humble nature of the objects he would cultivate. The humblest flower or coarsest weed may contain lessons of wisdom more profound than can be drawn from the most complicated conditions of life or of mind.

The city of Washington is becoming more and more a center, not only of scientific learning and research, but also of art and every form of liberal culture. Already the public schools have reached out and taken Botany into their curriculum, and we have seen that as a field for the pursuit of this branch of science the environs of the National Capital are in a high degree adapted. Science and culture must go hand in hand. Culture must become more scientific, and science more cultured. Botany has an important part to perform in this work of reconciliation, and there is no good reason why Washington may not become one of the foci from which these influences are to radiate. It has been such reflections as these, aside from the practical needs for such a work, that have encouraged me to persevere in this humble, indeed, but not the less laborious task, and if it shall be found useful to however slight a degree, in promoting these worthy objects, no regrets will arise at having undertaken it.

SUMMARY.

No.	ORDERS.	Genera.	Species.	Varieties.	Species and Varieties.	Introduced Plants.	Woody Plants.	Trees.
1	Ranunculaceæ	7	23	4	27	3	...	...
2	Magnoliaceæ	2	2	...	2	...	2	2
3	Anonaceæ	1	1	...	1	...	1	1
4	Menispermaceæ	1	1	...	1	...	1	...
5	Berberidaceæ	4	4	...	4	1	1	...
6	Nymphæaceæ	3	3	...	3	...	...	...
7	Sarraceniaceæ	1	1	...	1	...	...	...
8	Papaveraceæ	3	3	...	3	2	...	...
9	Fumariaceæ	3	3	...	3	1	...	...
10	Cruciferæ	16	32	1	33	15	...	...
11	Cistaceæ	2	2	...	2	...	...	...
12	Violaceæ	2	9	5	14	...	...	...
13	Polygalaceæ	1	7	...	7	...	...	...
14	Caryophyllaceæ	9	19	...	19	8	...	...
15	Illecebraceæ	2	2	1	3	...	...	...
16	Portulacaceæ	2	2	...	2	1	...	...
17	Hypericaceæ	3	9	...	9	1	1	...
18	Malvaceæ	4	7	...	7	5	...	...
19	Tiliaceæ	1	1	...	1	...	1	1
20	Linaceæ	1	3	...	3	1	...	...
21	Geraniaceæ	4	9	...	9	3	...	...
22	Rutaceaæ	2	2	...	2	1	2	...
23	Ilicineæ	1	4	...	4	...	4	1
24	Celastraceæ	2	3	1	4	...	4	...
25	Rhamnaceæ	1	2	...	2	...	2	...
26	Vitaceæ	2	6	...	6	...	6	...
27	Sapindaceæ	3	5	...	5	...	5	4
28	Anacardiaceæ	1	6	...	6	...	6	1
29	Leguminosæ	24	55	2	57	13	4	3
30	Rosaceæ	15	43	3	46	12	30	8
31	Saxifragaceæ	8	9	...	9	3	5	...
32	Crassulaceæ	2	3	...	3	...	...	...
33	Droseraceæ	1	1	...	1	...	...	...
34	Hamamelaceæ	2	2	...	2	...	2	1
35	Halorageæ	3	3	...	3	...	...	...
36	Melastomaceæ	1	1	...	1	...	...	...
37	Lythraceæ	4	4	...	4	...	...	...
38	Onagraceæ	6	10	1	11	...	...	...
39	Passifloraceæ	1	2	...	2	1	...	...

SUMMARY.—*Continued.*

No.	ORDERS.	Genera.	Species.	Varieties.	Species and Varieties.	Introduced Plants.	Woody Plants.	Trees.
40	Cucurbitaceæ	1	1	...	1	...	...	...
41	Cactaceæ	1	1	...	1	...	...	...
42	Ficoideæ	1	1	...	1	...	...	...
43	Umbelliferæ	17	22	...	22	2		...
44	Araliaceæ	1	4	...	4	...	1	1
45	Cornaceæ	2	5	...	5	...	5	2
46	Caprifoliaceæ	5	12	...	12	3	10	1
47	Rubiaceæ	5	12	1	13	...	1	...
48	Valerianaceæ	2	4	...	4	1	...	...
49	Dipsaceæ	1	1	...	1	1	...	...
50	Compositæ	53	138	11	149	17	1	...
51	Lobeliaceæ	1	5	...	5	...	...	...
52	Campanulaceæ	2	2	...	2	...	...	...
53	Ericaceæ	11	24	2	26	...	17	2
54	Primulaceæ	5	8	2	10	2	...	...
55	Ebenaceæ	1	1	...	1	...	1	1
56	Oleaceæ	2	4	...	4	...	4	4
57	Apocynaceæ	2	2	1	3	1	...	...
58	Asclepiadaceæ	4	13	1	14	...	...	...
59	Gentianaceæ	4	6	...	6	...	...	...
60	Polemoniaceæ	2	6	...	6	...	...	...
61	Hydrophyllaceæ	3	4	...	4	...	...	...
62	Borraginaceæ	7	12	...	12	3	...	...
63	Convolvulaceæ	3	11	...	11	4	...	...
64	Solanaceæ	5	8	...	8	5	...	...
65	Scrophulariaceæ	15	32	...	32	5	...	...
66	Orobanchaceæ	4	4	...	4	1	...	...
67	Lentibulaceæ	1	2	...	2	...	...	...
68	Bignoniaceæ	2	2	...	2	1	2	1
69	Acanthaceæ	2	3	1	4	...	...	...
70	Verbenaceæ	3	6	...	6	1	...	...
71	Labiatæ	23	41	1	42	10	...	...
72	Plantaginaceæ	1	5	1	6	2	...	...
73	Amarantaceæ	2	5	...	5	4	...	...
74	Chenopodiaceæ	3	7	2	9	7	...	...
75	Phytolaccaceæ	1	1	...	1	...	...	...
76	Polygonaceæ	3	21	2	23	7	...	...
77	Podostemaceæ	1	1	...	1	...	...	...
78	Aristolochiaceæ	2	2	...	2	...	...	...
79	Piperaceæ	1	1	...	1	...	...	...
80	Lauraceæ	2	2	...	2	...	2	1
81	Thymelaceæ	1	1	...	1	...	1	...
82	Santalaceæ	1	1	...	1	...	...	...
83	Loranthaceæ	1	1	...	1	...	1	...
84	Euphorbiaceæ	4	9	...	9	1	...	...
85	Urticaceæ	11	13	...	13	4	6	6
86	Platanaceæ	1	1	...	1	...	1	1
87	Juglandaceæ	2	7	...	7	...	7	7
88	Myricaceæ	1	1	...	1	...	1	...

SUMMARY.—*Continued.*

No.	ORDERS.	Genera.	Species.	Varieties.	Species and Varieties.	Introduced Plants.	Woody Plants.	Trees.
89	Cupuliferæ	7	25	1	26	...	26	23
90	Salicaceæ	2	14	5	19	7	19	6
91	Ceratophyllaceæ	1	1	...	1	...	...	...
92	Araceæ	5	6	...	6	...	...	...
93	Lemnaceæ	1	1	...	1	...	...	...
94	Typhaceæ	2	3	1	4	...	...	...
95	Naiadaceæ	2	9	...	9	...	...	...
96	Alismaceæ	2	3	2	5	...	...	...
97	Hydrocharidaceæ	2	2	...	2	...	...	...
98	Orchidaceæ	12	23	1	24	...	...	...
99	Amaryllidaceæ	1	1	...	1	...	...	...
100	Haemodoraceæ	1	1	...	1	...	...	...
101	Iridaceæ	2	6	...	6	1	...	...
102	Dioscoreaceæ	1	1	...	1	...	...	...
103	Smilaceæ	1	6	...	6	...	4	...
104	Liliaceæ	18	24	...	24	5	...	...
105	Juncaceæ	2	8	7	15	...	...	...
106	Pontederiaceæ	3	3	...	3	...	...	...
107	Commelynaceæ	2	3	...	3	...	...	...
108	Xyridaceæ	1	1	...	1	...	...	...
109	Eriocaulonaceæ	1	1	...	1	...	...	...
110	Cyperaceæ	10	94	14	108	...	...	...
111	Gramineæ	43	104	6	110	26	...	...
112	Coniferæ	4	7	...	7	1	7	7
113	Equisetaceæ	1	2	...	2	...	...	...
114	Filices	16	29	1	30	...	...	...
115	Ophioglossaceæ	2	2	2	4	...	...	...
116	Lycopodiaceæ	2	5	1	6	...	...	...
117	Musci	42	98	...	98	...	...	...
118	Hepaticæ	23	29	...	29	...	...	...
119	Characeæ	2	4	...	4	...	...	...

RECAPITULATION.

Groups.	Orders.	Genera.	Species.	Varieties.	Species and Varieties.	Introduced Plants.	Woody Plants.	Trees.
Polypetalæ	45	174	338	18	356	73	83	25
Gamopetalæ	27	169	368	21	389	57	36	9
Dichlamydeæ	72	343	706	39	745	130	119	34
Monochlamydeæ	19	47	114	10	124	30	64	44
Dicotyledones	91	390	820	49	869	160	183	78
Monocotyledones	20	112	300	31	331	32	4	---
Gymnospermæ	1	4	7	----	7	1	7	7
Phænogamia	112	506	1,127	89	1,207	193	194	85
Vascular Cryptogamia	4	21	38	4	42	----	----	----
Vascular Plants	116	527	1,165	84	1,249	193	194	85
Cellular Cryptogamia	3	67	131	----	131	----	----	----
Total Flora	119	594	1,296	84	1,380	193	194	85

On this communication, Mr C. A. White remarked that he hoped Mr. Ward would be able to furnish some further information concerning the influence exerted upon a flora by the character of the country rocks. It is well known that the constitution of the strata, influencing as it does the character of the soils which cover them, had a further effect upon the native plants growing above them. Thus the granite localities of the east were more favorable to the growth of certain genera, for example, the Ericaceæ than the magnesian limestones of the Mississippi valley. He hoped that Mr. Ward might be able to ascertain how far these influences affected other families of plants.

Mr. Powell inquired what were the characters or character of plants that had apparently disappeared from the local flora in the comparison of the field results of the present time with those obtained forty or fifty years ago.

Mr. Ward replied that the missing species in the present lists were not confined to any particular family, but were diffused considerably among the several classes.

The Society then adjourned.

193D MEETING. FEBRUARY 5TH, 1881.

Vice President WELLING in the Chair.

Thirty-eight members present.

The minutes of the last meeting were read and adopted.

A communication was then read by Mr. C. E. DUTTON, on

THE SCENERY OF THE GRAND CANON DISTRICT.

The communication was reserved by the author.

Remarks upon this communication were made by Mr. J. W. POWELL, at the conclusion of which, the Society adjourned.

194TH MEETING. FEBRUARY 19TH, 1881.

Vice President TAYLOR in the Chair.

Thirty-one members present.

The minutes of the last meeting were read and adopted.

The President announced to the Society the death of Dr. GEORGE A. OTIS. It was moved and carried, that a committee be appointed to prepare suitable resolutions for the action of the Society, relative to the death of Dr. OTIS, and the Chair appointed a committee consisting of Messrs. Antisell, Billings, and Mew.

The first communication for the evening was by Mr. J. E. TODD, of Iowa who had been invited by the General Committee to read a communication on the

QUARTERNARY DEPOSITS OF WESTERN IOWA AND EASTERN NEBRASKA.

Mr. TODD gave first an account of the three members which compose the Quarternary deposits of the regions in questions. The lowest is in Iowa, and is the boulder-clay consisting of the hard compact clay usually occurring in this formation, with its included rocky glaciated fragments. In central and western Nebraska this clay is wanting. Upon it rests the red clay, a formation of varying thickness, but usually quite thin, rarely exceeding 20 feet. Upon this rests the *loess* which constitutes a subject of special interest. One peculiarity of it is found in the fact, that it overlies the inequalities of the country which existed prior to its disposition; being found upon the old hill tops and slopes, as well as in the valley bottoms, and exhibiting a general "unconformity by erosion." It is composed of exceedingly fine matter without any fragments of rock of notable size, such as pebbles or stones. It contains, however, bands of calcareous concretions in lines which are usually horizontal, and these concretions are often elongated with their longer dimensions vertical. It also holds those calcareous fibres which Richthofen observed in the *loess* deposits of China, and which he believed to be casts of roots of plants. Another interesting occurrence is that of charcoal, which is found in several places in the midst of the deposits in thin bands. The fossils of the loess are the shells of geophilous mollusca.

Mr. TODD held the view that the loess is a post-pliocene lacustrine deposit, and that the region in discussion was in post-glacial time covered with a very large fresh-water lake.

Prof. T. C. CHAMBERLAIN, of Wisconsin, being present, and invited to take part in the discussion, remarked that while Mr. Todd had presented in a very able and clear manner the reasons for attributing the loess to the deposit of silt in a lake bottom, he was of opinion that the objections to the acceptance of that view were very great. If such a lake existed over the region in question during quarternary time, it must have been of immense extent. According to the observations of Dr. C. A. White, these deposits extend to the borders of the region which drains immediately into the Mississippi river in Iowa, and they are found nearly as far west as the Rocky Mountains. Their north and south extensions are not accurately known, but they are believed to be very great. Independently of these deposits no evidences of such a lake are now known. Its boundaries are not marked by any known barriers on the east where the configuration of the couutry is now such that no barriers could have existed, unless the region which they should have occupied has undergone remarkable changes of which the nature cannot be specified, and of which no traces exist. To produce such a lake basin very great depressions would be necessay, and there is no evidence known to him which warrants a belief in a former depressed condition of that region sufficient to account for it. Further research may indeed relieve us of some of these difficulties or all of them, but at present they are very great. Prof. Chamberlain could not but commend, however, the earnest and scientific spirit in which Mr. Todd had pursued his valuable investigations.

Mr. O. T. Mason inquired whether the occurrences of charcoal were frequent and bore evidence of human agency.

Mr. Todd replied that charcoal was often met with, and suggested as a possible, though not probable, explanation, that the fragments may have come from some of the recent volcanic regions of the west.

Mr. C. E. Dutton suggested that there would be little difficulty in finding a natural cause for the occurrence of charcoal, if the surface had been above water at the time it was deposited. There can be little doubt that fires are frequently started in the woods and on the plains of the west by lightning, and it is not at all incredible that they may sometimes arise from spontaneous ignition. Many of the frequent fires in the western mountains occur under circumstances which render it incredible that human agency was involved.

Mr. C. A. White spoke of the great areas over which loess deposits are found. They occur not only in the upper Mississippi valley, but also in the regions of the lower Mississippi. They also occupy a great range of altitudes, some being only a few hundred feet above the level of the sea, others several thousand feet above it. They all seem to be of similar character and constitution. The absence of any barriers is one powerful argument against the existence of a lake, and the great changes of level which would be demanded to establish this hypothesis is another.

The next communication was read by Mr. C. E. Dutton, on

THE VERMILION CLIFFS AND VALLEY OF THE VIRGEN, IN SOUTHERN UTAH.

The paper was reserved by the author.

At its conclusion the Society adjourned.

195th Meeting. March 5th, 1881.

Vice-President Taylor in the Chair.

Twenty-two members present.

The minutes of the last meeting were read and adopted.

The Chair announced the election of Mr. Peter Winfield Lauver to membership in the Society.

The first communication was by Mr. THEODORE GILL on the

PRINCIPLES OF MORPHOLOGY.

Mr. Gill's paper may be found substantially in Johnson's Encyclopœdia, under the title Morphology, which article was written by him.

The second communication was by Mr. MARCUS BAKER on the

BOUNDARY LINE BETWEEN ALASKA AND SIBERIA.

The present boundaries of the territory of Alaska were defined in the treaty of March 30, 1867, whereby Russian America was ceded to the United States. In that treaty the western boundary, or rather so much of it as is here considered, was defined as follows:

"The western limit, within which the territories and dominion conveyed are contained, passes through a point in Behring's Straits on the parallel of sixty-five degrees thirty minutes north latitude, at its intersection by the meridian which passes midway between the island of Krusenstern or Ignalook, and the island of Ratmanoff or Noonarbook, and proceeds due north without limitation into the same Frozen Ocean."

The longitude of this meridian was very properly left out of the treaty on account of its uncertainty. In order to show our knowledge of the subject at the time of the framing of the treaty the following table has been prepared from all known authorities upon the subject down to the present time.

The last three determinations entered in the table, it must be borne in mind, have been made since the treaty was drawn up.

Date.	Longitude.		Authority.
	°	′	
1761	155		Map published by the Imp. Acad. of Sc. of St. Petersb.
1778	169	52	Cook's Atlas.
1802	168	48	Billings.
1822	168	59	Kotzebue.
1827	168	55	Beechey. Br. Adm. Ch. No. 593.
1828	168	54	Lütke's Atlas.
1849	168	57.5	Febenkoff's Atlas.*
1852	168	54	Russian Hydr. Ch. No. 1455.
1855	168	48	Rogers. U. S. Hyd. Ch. No. 68.
1874	169	04	Russ. Hyd. Ch. No. —.*
1878	168	58	Onatsevich.
1880	168	58	U. S. C. and G. S.

In the case of the two determinations marked with a * the two Diomede Islands are so represented on the chart that the boundary line is tangent to each island.

During the past summer an attempt was made by the party on board the U. S. C. and G. S. Schooner Yukon to make a more careful determination of the longitude of this meridian than had been attempted hitherto. For longitude purposes the party had one pocket and six box chronometers. For determining time the sextant was used, recourse being had to equal altitudes whenever possible.

Plover Bay in Eastern Siberia is about 150 miles to the southward and westward from the Diomede Islands in Behring's Strait. This bay was visited by Prof. Asaph Hall of the U. S. Naval observatory in 1869 for the purpose of observing the total solar eclipse of that year, and, in connection with the eclipse work, Prof. Hall made a careful determination of the longitude of his station. After a careful examination of all the longitude determinations known to exist, and because the facilities for determining the longitude of this place by the Yukon party were not sufficient to improve upon the determination by Prof. Hall, his results have been adopted, and the longitude of the boundary meridian made to depend upon his determination. Before proceeding to give an account of our longitude observations, when near the boundary line, a complete *resumé* of observations for position at Plover Bay, with discussion will be given, this being rendered necessary by the fact that the longitude of the boundary line as well as that of all other points along the Arctic coast and northern part of Behring Sea have been made by us to depend upon Plover Bay.

Previous to 1848 Plover Bay, though an extensive arm of the sea running inland some 20 to 25 miles, appears not to have been known. It is not shown upon any map before 1850. In the period from 1845 to 1848 it seems to have been visited by the whalers. The first information touching it upon which we can lay our hands is the report of Commander Moore to the Admiralty, published in the Nautical Magazine March, 1850. From this it appears that Commander Moore first anchored in Plover Bay, October 17, 1848 Later he moved his vessel, the Plover, farther in, and wintered in the harbor named by him Emma Harbor. He remained in Emma Harbor until June 23, 1849. Concerning the scientific or surveying work accomplished in this period of eight months, he says; "At intervals Mr. Martin, assisted by Mr. Hooper, made a survey of the place in which I had secured the ship for the winter; which, connected with Mr. Martin's and my own observations on the coast to the westward, will, I hope, give a tolerably correct representation of these shores, and when associated with magnetic observations on every attainable point, will, I trust meet their Lordships' approbation."

The results foreshadowed by this report have not come to light. No map or plan of Emma Harbor, or Plover Bay, has been published by the British Admiralty Office, and no statement or account of the observations at Plover Bay, if any were made. General Sabine in his contributions to Terrestial Magnetism No. XIII gives some results which he credits to a MS in the Magnetic Office by Commander Moore, but no *magnetic declination* or *intensities* are given; whence we conclude that no observations, or at least no satisfactory observations, therefore, were taken. A few results for *dip* are given. The geographical position of the station where the dip observations were taken is given by General Sabine, and this position, if due to Commander Moore, is the earliest determination on record of a position for Plover Bay. The position given probably refers to some point near the northern shore of Emma Harbor and is

Latitude,	64°	26′	N.
Longitude,	173	07	W. Gr.

and the observed dip was 75° 10′. From the best existing chart of Plover Bay that we have, it is found that this station is four minutes north, and nine minutes east of the station occupied by the Coast Survey. Whence we find the Coast Survey Astronomical Station to be, according to Commander Moore, approximately in

Latitude,	64°	22′	N.
Longitude,	173	16	W. Gr.

A rough sketch of Plover Bay was made in 1866, by the exploring parties of the Western Union Telegraph Company, and this sketch was published in 1869 by the Coast Survey. The observations were made by Lieut. J. Davidson, of the U. S. Revenue Marine Service, and the resulting position is stated to depend upon nine observations referred by a crude triangulation to the mountain Bald Head. The position given by Lieut. Davidson for Bald Head is

Latitude,	64°	24′	N.
Longitude,	173	15	W. Gr.

From the best chart extant of Plover Bay, which has been referred to above, and which is one published in 1877 by the Russian Hydrographic Office from surveys by Lieut. Onatsevich, we find Bald Head to be one and a half minutes south and one minute east of the Coast Survey Astronomical Station. Hence, according to Lieut. Davidson, the Coast Survey Astronomical Station is in

Latitude, 64° 25.′5 N.
Longitude, 173 16 W. Gr.

As the observations were made, *not* on the mountain, but on the vessel at anchor in the harbor, it seems probable that in transferring the position of the vessel to the mountain some mistake occurred, for the resulting latitude is certainly considerably in error.

The next determination of position at Plover Bay was by Prof. Hall, in 1869, during his visit to this place to observe the total solar eclipse of that year. The latitude was determined with a Pistor and Martin's sextant from observations upon August 3, 4, and 5, by Prof. Hall and Mr. J. A. Rogers. The following table gives the results:

Date.	Latitude.			Observer.	No. of Observations.
	°	′	″ ″		
1869, August 3	64	22	22 ± 1.3	Rogers	15
" 3			22 ± 1.9	Rogers	14
" 4			33 ± 1.9	Hall	17
" 5			27 ± 1.9	Hall	12
" 5			20 ± 2.7	Hall	12
Mean adopted	64	22	25		70

For determining the longitude Prof. Hall had ten chronometers whose corrections to Greenwich time were determined at the Astronomical Station in the Navy Yard on Mare Island, California, before setting out and returning from Plover Bay. The dates of the time determinations at Mare Island, are June 17–20, and September 18–19, 1869, the interval being 102 days. The time was determined with a small portable transit instrument. With these means Prof. Hall obtained the following results for the longitude of his station in Plover Bay, west from the station at Mare Island.

h.	*m.*	*s.*
3	24	21.3
		19.1
		21.3
		21.0
		22.7
		22.2
		22.5
		15.9
		23.0
		21.1

These are the results by each chronometer, and when combined by weights indicated by their probable errors, the resulting longitude is

h.	*m.*	*s.*	*s.*
3	24	21.1 ±	0.36

Since these results were published, the longitude of San Francisco has been determined by telegraph, and the station upon Mare Island occupied by Prof. Hall geodetically connected with this determination. The resulting longitude of the Mare Island station is, according to Assistant Schott of the Coast Survey,

°	′	″	″
122	16	16 ±	2.2

or, in time,

h.	*m.*	*s.*	*s.*
8	09	05.07 ±	0.15

whence we have for the longitude of Prof. Hall's station, at Plover Bay

h.	*m.*	*s.*	*s.*
11	33	26.2 ±	0.4.

For Prof. Hall's station, therefore, we adopt

Latitude,	64°	22′	25″	N.
Longitude,	173	21	33 ± 6″	W. Gr.

Before leaving Washington we were furnished by Prof. Hall with a memorandum, describing his station from which it appears that no permanent station mark could be left by him, the character of the soil and natives preventing this. We were, therefore, unable to locate the exact spot, but had no difficulty in finding the general locality, and fixing upon a place that must have been within a few metres of Prof. Hall's station. Here we erected a pile of boulders as a beacon, and by means of the telemeter staff, and a small triangulation connected with our azimuth line, we found this beacon to bear N. 1° 42′ 26″ E. from our astronomical station, and 462.9 metres distant, or in round numbers 460 metres N. 1° 42′ E. of ours; in arc this is 1″ E. and 15″ N. of ours. Applying these reductions to the position already adopted, we have as the position of our station, according to Prof. Hall

Latitude,	64°	22′	10″	N.
Longitude,	173	21	32 ± 6″	W. Gr.

In 1876 the bay was visited by Lieut. M. S. Onatsevich, of the Russian Navy in the "*Vsadnik*," and a rough survey made of the bay with a somewhat detailed survey of the anchorages. At the same time astronomical and magnetic observations were made.

In 1877, the Russian Hydrographic Office published several charts embodying the results of Onatsevich's observations, and among them, a chart of Port Providence, or "Plover Bay," as it is usually called by the whalemen. On this chart it is stated that the astronomical station of Lieut. Onatsevich is, according to his observations in

Latitude,	64°	21′	37″	N.
Longitude,	173	18	30	W. Gr.

In the following year, however, 1878, Lieut. Onatsevich's report was published, and in this report the position of the astronomical station is stated to be

Latitude,	64°	21′	55″	N.
Longitude,	173	23	54	W. Gr.

the longitude depending upon that of Petropavlovsk, which latter is taken as 10h. 34m. 37s. or 158° 39′ 15″ E. from Greenwich. This last result appears to be the finally corrected one, and is adopted as Onatsevich's determination.

The station occupied by Lieut. Onatsevich is clearly marked upon his chart, and as we had this chart with us the place was quite closely identified, probably within a few feet. The attempt was made to have our station identical with his, and consequently no reduction is necessary.

Recapitulating, therefore, we have the following results for the position of the Coast Survey Astronomical Station at Plover Bay:

Date.	Latitude.			Longitude.			Authority.
	°	′	″	°	′	″	
1848–9	64	22		173	16		Com'r T. E. L. Moore. (?)
1866		25.5			16		Lieut. J. Davison.
Aug., 1869		22	10		21	32	Prof. A. Hall.
July, 1876		21	55		23	54	Lieut. M. L. Onatsevich.
Sept., 1880		21	54				U. S. C. and G. S., by M. Baker.

Discussion of foregoing Table.

It is very doubtful whether the results credited to Commodore Moore were really obtained by him, or whether General Sabine took these values from other sources; while the results by Lieut. Davison are known to have been of only a very approximate character. The three remaining results for latitude, when we consider that they were made at different times, by different observers, at different stations, and with different instruments and the instruments of a secondary character, show a satisfactory agreement, and we adopt the simple mean for the latitude determination, which is 64° 22′ 00″ and would assign an arbitrary probable error of 6″.

Neglecting the longitude results by Moore and Davison as being of an inferior character, we have the two remaining by Hall and Onatsevich. The determination by Onatsevich is a chronometric one from Petropavlovsk. How the longitude of Petropavlovsk was obtained we are not informed, but we know it was not determined by telegraph. Moreover the longitude adopted by Onatsevich for Petropavlovsk differs by as much as four miles, (4′ 11.7″ = 16.8s) from that adopted by the Russian Hydrographic Office, in 1850, as the basis for their charts of this region, and which determination was the mean of nine different determinations, extending from 1779 to 1827. The longitude of Plover Bay based upon Onatsevich's observations and that longitude of Petropavlovsk is 173° 19′ 22″ W. Gr.

It has, therefore seemed best to adopt without change the result of Prof. Hall's observations, not combining it with anything else, viz: 173° 21′ 32″ ± 6″ W. Gr.

Our adopted value, therefore, of the geographical position of the Astronomical Station of the U. S. Coast and Geodetic Survey at Plover Bay, Eastern Siberia, is

Latitude,	64°	22′	00″	± 6″	N.
Longitude,	173	21	32	± 6	W. Gr.
	h.	m.	s.	s.	
	11	33	26.1	± 0.4	

One station was marked by driving a piece of whale's rib into the ground and piling rocks around it. Being identical with the station of Lieut. Onatsevich, any one visiting the place will by the aid of that chart readily identify it.

Having completed our investigation of the geographical position of Plover Bay, we proceed to detail our observations for the longitude of the boundary.

The Yukon arrived at Plover Bay at ten in the evening of August 11, 1880. The following day was cloudy in the morning, afterward rained, and later partially cleared up so that we obtained two pairs of equal altitudes of the sun for time, the interval being about three hours. During the afternoon we succeeded in getting four sets of six each of double altitudes of the sun for time. From the equal altitudes the time of local mean noon by the chronometer, was 11h. 18m. 13.9s, and from the double altitude it was 11h. 18m. 14.2s., a very satisfactory agreement. By means of the intervals the probable errors of each of these determinations have been made out. For the equal altitudes it is ± 1.7s, and for the double altitudes it is ± 0.30s, values which may be taken as fairly representative of the different conditions under which the observations were made. From these observations the corrections of our chronometers to Greenwich mean time on August 12 were determined.

On August 14, we sailed from Plover Bay to the eastward and northward, cruising along the Arctic coast as far as Point Belcher, and returning thence passed through Behring Strait to Port Clarence, and afterwards returning to Behring Strait made a landing on the southeastern shore of Ratmanoff, or the Big Diomede Island, on September 10. We came to anchor at seven in the morning, about a mile off shore, and sailed away about three in the afternoon. During our stay observations were made for latitude and time, and all the magnetic elements, declination, dip and intensity. Of time observations three sets of six each of double altitudes of the sun were obtained with sextant and artificial horizon. These three sets give as the correction of our "hack," or observing chronometer, to local mean time

h.	*m.*	*s.*	*s.*
+ 1	03	26.9	± 0.35,

this probable error resulting from computing the eighteen observations singly and treating in the usual way. The sky was nearly covered with cumulus clouds, the wind fresh, raw and chilly, and thermometer 39° F. Near noon the sun appeared again for a short time, and nine pointings were obtained for latitude, giving the following results, each depending upon a single observation.

	65°	44′	54″
			50
			38
			54
			44
			52
			53
			60
			65
Mean latitude,	65°	44′	51 ± 1.″5 N.

Leaving the Diomedes on the afternoon of September 10, we sailed directly for Plover Bay.. That night we were stopped by ice, the next day delayed by calms, but on the following day, September 12, we reached our anchorage in Plover Bay a little before noon, just in time to get a good series—39 observations of circummeridian altitudes of the sun for latitude. In the afternoon we obtained a good series of time observations, but the following morning was cloudy. We succeeded, however, in getting four altitudes corresponding to those of the preceding day, thus enabling our time determination to hang upon four pairs of equal altitudes, the epoch being local mean midnight September 12 and 13. The times of local apparent midnight from these four pairs by our "hack" were

h.	*m.*	*s.*
11	09	0.2
		1.2
		0.3
		0.7

from which the probable error is found to be ± 0.15*s*.

For the longitude of our station upon the Big Diomede Island we have, therefore, as follows:

Plover Bay	1880, Aug.	12, noon	Chron'r corr'n determined,	± 1.7 *s.*
Big Diomede Id.,	" Sept.	10, 8.9 *h.* a. m.,	" "	± 0.35
Plover Bay	" "	12, midnight	" "	± 0.15

By means of the time determinations of August 12 and September 12, the rates of the chronometers are determined and then the Greenwich time determination at Big Diomede Island, September 10, is made to depend upon the determination at Plover Bay, September 12, and the rates of all the chronometers carried back to September 10, a period of 2.64 days.

The resulting longitude by each chronometer is shown in the following table:

Chron'r.	*h.*	*m.*	*s.*
214	11	16	18.3
866			17.9
1131			18.0
1713			19.0
2535			14.7
311			16.6

Chronometer No. 2535 was our "hack," and 311 a sidereal chronometer used in making comparisons. Each had rather large rates, that of 2535 exceeding *nine* seconds, and that of 311 *five* seconds per day. The indiscriminate mean of all is 11*h.* 16*m.* 17.4*s.* Assigning only half weight to chronometer 2535, the longitude resulting is

h.	*m.*	*s.*
11	16	17.7

The probable error of the Greenwich time at the Diomedes, based upon the agreement of the chronometer is ± 0.36*s.*

For the probable error of the longitude, therefore, we have

Probable error of longitude of Plover Bay	= ± 0.39 *s.*
Probable error local time determination, Plover Bay, Sept. 12	= ± 0.15
Probable error local time determination, Diomedes, Sept. 10	= ± 0.35
Probable error Greenwich time determination, Diomedes, Sept. 10	= ± 0.36

	h.	*m.*	*s.*	*s.*
Resulting longitude adopted,	11	16	17.7	± 0.65.

The astronomical station of the United States Coast and Geodetic

Survey at the mouth of the ravine, on the southeastern shore of the Big Diomede Island, in Behring Strait, is, therefore, in

Latitude,	65°	44′	51″	N.
Longitude,	169	04	25 ± 10	W. Gr.

From bearings and angles taken from the astronomical station and from the schooner at anchor, using the distance of the schooner from the station as a base line, together with other bearings taken while in the vicinity of the islands, a sketch of the two islands has been prepared from which it appears that the meridian tangent to the extreme eastern edge of the larger island is 2.1 nautical miles, and the meridian tangent to the extreme western edge of the smaller island is 3.1 nautical miles, east of the astronomical station. The boundary line is to pass midway between these meridians, *i. e.* the meridian which forms the boundary is 2.6 nautical miles east of the astronomical station.

In latitude 65° 45′, the latitude of the astronomical station, 2.6 nautical miles is equal to 6′ 20″ of longitude, and, deducting this from the longitude of the astronomical station, the longitude of the boundary line is found to be

168° 58′ 05″ W. Gr.

If we assume an uncertainty of one quarter of a nautical mile, equal in this latitude to 37″ of longitude, in thus transferring the position of the station to the boundary line, and this seems to be quite large enough, we have finally as the longitude of the boundary line between Alaska and Eastern Siberia

°	′	″	″
168	58	05 ±	38

or, in time,

h.	*m.*	*s.*	*s.*
11	15	52.3 ±	2.5 W. Gr.

196TH MEETING. MARCH 19, 1881.

Vice-President TAYLOR in the Chair.

Thirty members and visitors present.

The minutes of the last meeting were read and adopted.

The communication for the evening was by Mr. J. W. POWELL, on

LIMITATIONS TO THE USE OF SOME ANTHROPOLOGIC DATA.

This paper is published in full in the "Abstract of Transactions of the Anthropological Society of Washington, D. C., for the first year ending January 20, 1880, and the second year ending January 18, 1881."

Remarks upon this communication were made by Messrs. GILL, HARKNESS, WARD, NEWCOMB, and ALVORD.

At the conclusion of the discussion the Society adjourned.

197TH MEETING. APRIL 2D, 1881.

Vice-President TAYLOR in the Chair.

Thirty-nine members and visitors present.

The consideration of the minutes of the last meeting was postponed, the recorder being absent.

Dr. ANTISELL, on behalf of the committee appointed at the last meeting of the Society, reported the following resolution in commemoration of the late Dr. GEORGE A. OTIS:

Resolved, That this Society has heard with profound regret of the untimely death, on the 23d of February last, of Dr. GEORGE A. OTIS, U. S. Army, one of its original founders.

Resolved, That while we deplore the loss of so highly valued an associate and friend, there is some compensation to be found in the reflection that his long and incessant suffering has at last terminated, and that it is gratifying to remember that he was not cut off before his services to science, in his chosen field, had received, as well in Europe as in America, the high appreciation which they so richly merited.

Resolved, That the medical literature, not only of this country but of the world, has sustained by this calamity a loss which can with difficulty be replaced.

A communication was then read by Mr. A. B. Johnson on

THE HISTORY OF THE LIGHT HOUSE ESTABLISHMENT OF THE UNITED STATES.

Mr. Johnson read from a paper he had prepared for publication elsewhere, on the History of the Light-house Establishment of the United States, tracing its rise and progress from the first beacon which was erected on Point Allerton, entrance to Boston Harbor, in 1673, to the present time. He gave some account of the eight light-houses built by the Colonies; then of twelve built by the General Government prior to 1812, then of the progress of the establishment, under the charge of Mr. Pleasanton, an Auditor of the U. S. Treasury and the Acting Superintendent of the Lights, when the number increased to some three hundred and twenty-five; then of the causes which led to the creation of the provisional Light-House Board, and then of the erection of the permanent Light-House Board, and of the improvements the Board had since made, in all the arts and sciences connected with the erection of the light-houses and the establishment of cognate aids to navigation. Mr. Johnson then gave some account of light-house construction and of the different kinds of light-towers, material and style of the structures used, and of the problems solved in deciding on the various subaqueous foundations required. He illustrated his subject by the exhibition of large photographs of such stone light-houses as that on Spectacle Reef, Michigan, of such harbor lights as that on Thimble Shoal, entrance to Hampton Roads, Virginia, such skeleton iron houses on driven piles as that on Fowey Rocks, Florida Reef, and the tripod erected on Paris Island, Port Royal Sound, S. C., and of the remarkable stone light-house recently built on the summit of Tillamook Rock off the coast of Oregon.

Some account was given of the fog-signals used in this country, and a large crayon of the syren, the most powerful fog signal known, was shown.

Mr. Johnson spoke of the fact thus noted by Professor Henry: "It frequently happens on a vessel leaving a station that the sound is suddenly lost at a point in its course, and after remaining inaudible some time, is heard again at a greater distance, and is then gradually lost as the distance is further increased." In connection with this he exhibited a chart showing the site of Beaver Tail Light-House on the south point of Conanicut Island, between the two entrances to Narragansett Bay, with Bonnet Point, on which the steamer Rhode Island was wrecked in the fall of 1880, one and one-half miles to the northwest, with Fort Adams three and one-quarter miles to the northeast, and distant one and one-half miles to the southeast. On this chart was indicated the route of a sail boat which had been run to Bonnet Point, thence southerly to near Whale Rock; thence easterly close to Beaver Tail; thence northeasterly to Fort Adams, and thence southeasterly to Newport. On the route followed by the boat, he had indicated by half inch circles, the audibility of the fog-signal in full blast at Beaver Tail, as heard in the boat; the degrees being shown by the various shades; full audibility being indicated by darkening the whole surface of the circle, and complete inaudibility being shown by lack of shading in the circle. In this way it was shown that the observer, an officer of the Navy, found the sound of the fog-signal faint at half a mile from the signal, fainter at three-fourths of a mile off, much louder at a mile, less loud at one and one-eighth miles; he lost the sound entirely at one and one-fourth miles; at one and three-sixteenths miles he heard it faintly, and right under Bonnet Point, one and one-half miles distant, he heard it stronger than he did at one-half mile from the signal. In the run of about one mile from Bonnet Point toward Whale Rock he did not hear the fog-signal at all, and then he heard it faintly, and as he then ran almost toward the signal he lost its sound entirely; when about a half a mile west of the signal he heard its sound quite faintly, and then lost it, not hearing it again till within one-fourth of a mile when he suddenly heard it at its full power and continued to do so on his run to Newport until three-fourths of a mile away, when the sound diminished one-half, and continued so at one mile off and one and one-fourth miles off. At one and one-half miles distance the sound had diminished to about one-fourth of its power; at two miles off he lost it; he did not hear a trace of it at two and one-fourth, two and a half, or two and three-fourths miles distances; but he caught it faintly as he rounded Fort Adams at three miles away, and when he had run another one-fourth of a mile into Newport Harbor he heard it at almost its full power and continued to do so for another quarter of a mile, when he lost it all together.

Mr. Johnson called attention to the fact that in the run of this boat, the sound of the fog-signal had ranged from audibility to to inaudibility, and back again, several times; and that while it was lost at a distance of about a mile, it was distinctly, though faintly heard at Bonnet Point, distant one and one-half miles, and that while it was lost completely at two miles off, on the run to Newport, it was picked up at Fort Adams, three miles off, and heard almost at its full power at three and one-fourth and three and one-half miles away. These records were made by Lieut. Com. F. E. Chadwick, U. S. N., Assistant Light-House Inspector, to ascertain the facts, bearing on the statement that the fog-signal stopped from time to time, made by those who had noticed these intermissions of audibility; and the fact that the fog-signal was in continuous full blast, was noted by his assistant, who remained at Beaver Tail for the purpose.

Mr. JOHNSON stated that this ricocheting of sound, these intervals of audibility, ought to be recognized by the mariner, who should now understand that in sailing toward or from a fog-signal in full blast, he might lose and pick up its sound several times though no apparent object might intervene. And the mariner now needed that science should deduce the law of this variation in audibility and bring out some instrument which should be to the ears what the maiiner's compass is now to the eyes, and also that variations of this instrument yet to be invented, be provided for and corrected as now are the variations of the mariner's compass. The speaker referred to the benefit the mariner had derived from the promulgation of Professor Henry's theory of the tilting of the sound wave up or down by adverse or favorable winds, and said that by this the sailor had been led to go aloft in the one case and to get as near as possible to the surface of the water in the other, when trying to pick up the sound of a fog-signal.

In this connection Mr. JOHNSON read the following extract from an article entitled *Signaling by Means of Sound, by E. Price-Edwards*, from the [*English*] *Journal of the Society of Arts*:

"In one respect, however, the late Professor Henry, who was at the time chairman of the United States Light-House Board, differred from Dr. Tyndall, viz: in regard to the theory of acoustic clouds, and their resultant aërial echoes. Professor Henry's explanation of the obstruction of sound in clear weather, and the echoes, is founded upon the asserted existence of upper and lower currents of air, the tilting up of the sound wave, and the reflection of the sounds from the surface of the sea, or the crests of the wave. From this last explanation, Professor Henry seems to have receded before his death."

Mr. JOHNSON said that he called attention to this statement, as he was satisfied that Mr. Price-Edwards had permitted himself to fall into some inaccuracy as to Prof. Henry's action in this matter. It was within Mr. Johnson's personal knowledge that Prof. Henry, up to the last, had considered the theory of the tilting of the sound wave, under certain conditions, as a good working hypothesis. The Professor had it in contemplation when he was called from his labors to attempt the solution of certain of the questions connected with this subject by stationing observers in steamers, around a vessel anchored far enough from shore to be out of reach of land echoes, on which a powerful fog-signal should be in operation, and these observers should be aided by others in captive balloons, who should note simultaneously with them, upon charts and tables previously prepared, not only the audibility of the signal, but all the other data which could be obtained from the action of the thermometer, the hygrometer, and the anemometer, as to the then condition of the atmosphere. When all this information should be tabulated, Professor Henry hoped to deduce something more of the law of the movement of the sound wave under given conditions, and to formulate it for the benefit of the mariner. This was a work which Professor Henry had left to his successors and which the speaker believed they would not neglect.

Mr. JOHNSON then took up an article in the *Annales des Ponts et Chaussés for October*, 1880, *by M. Emile Allard, Inspecteur General des Ponts et Chaussés*, entitled *Comparison de Quelques Depenses Relative au Service des Phares en France, aux Etats-Unis et en Angleterre*, and called attention to that portion of it in which it was stated in effect, that the lighted coast of the United States measured about 7,500 nautical miles, and that the estimate of the Light-House Board of the expense of maintaining the Light-House Service for the year ending June 30, 1880, was $2,046,500, and that hence the cost to the United States for lighting each nautical mile of its coast was 1,293 francs, while that of France which had twenty-five lights to the one hundred nautical miles [the United States having but about nine lights to that distance] was but 1,155 francs.

Mr. Johnson then showed that the length of the lighted coasts of the United States, except those of the Mississippi, Missouri, and Ohio rivers, measured on a ten-mile chord, was 9,959 miles, giving, as his authority, recent statements made on this point by the United States Coast and Geodetic Survey and of the office of the Chief of Engineers of the United States Army; the one as to the length of the ocean, gulf, sound, and bay coast, and of the lighted rivers beside those above named, and the other as to the length of the lighted lake coasts. He then pointed out the natural mistake of M. Allard, in supposing that the amount of the Board's estimates (*Le Budget Annuel du Bureau des Phares*) had been appropriated by Congress for its support; and he showed instead that the appropriations were much less than the estimates, and that, owing to various causes, the appropriations even had not all been expended, so that the actual expenses of maintaining the United States Light-House Establishment for the year ending June 30, 1880, were but $1,943,600 instead of $2,046,500, as M. Allard had inferred. Hence, it followed that, while it costs France 1,155 francs to light each nautical mile of her coast, it costs but 922.7 francs to light each nautical mile of United States coast, instead of 1,293 francs as has been erroneously inferred by M. Allard.

Mr. Johnson closed by stating that the Light-House Establishment of the United States had been largely modeled on that of France; that the Light-House Board, while it still hoped to reach the French standard in many things, hardly expected to attain to certain of its economies; that he should not have thought of comparing the cost of the maintenance of the two establishments, but as this comparison had been made in the official French journal, he had thought it well, and due to the science of pharology, to correct the errors which had crept into the calculations of this high officer in the French Light-House Service.

The paper from which Mr. Johnson read, and on which he based his remarks, may be found in full in the Annual Appendix for 1880, to be published by the Appletons as Volume XX of the New American Cyclopedia.

Remarks on this paper were made by Messrs. Hilgard and Thornton A. Jenkins. The latter gave some interesting reminiscences of his early connection with the light-house service.

Mr. TAYLOR said that he wished to emphasize a single point in Mr. Johnson's communication, namely, that referring to Mr. Price-Edwards' statement in regard to the supposed change of view by Prof. Henry as to the explanation of acoustic disturbances, or, at least, as to the source of the ocean echo. The only thing which could give the slightest color to such a supposition was a purely incidental and wholly unimportant suggestion thrown out by Prof. Henry on this subject. Discarding the proposed explanation of the echo by the presence of a hygroscopic flocculence, or invisible acoustic clouds in the air, as quite insufficient in character, as too indefinite in limits, and as too mutable and evanescent in duration, in a mobile atmosphere, to account for so pronounced, distinct, and uniform a phenomenon, Prof. Henry thought, in the absence of any other sufficient surface, that, in view of the large amount of curvature in ordinary sound beams, acoustic waves might be reflected back to the ear from the ocean itself,—probably from the sloping sides of the waves. On having his attention drawn by Prof. Tyndall to the circumstance that the echoes were frequently distinct over a perfectly smooth sea, he admitted that this would invalidate the suggestion of wave crests being concerned in the effect; but he still believed that, with sounds sufficiently powerful to reach considerable distances, it was quite possible for some of the upper sound-beams to be so curved as to be reflected upward from a perfectly level floor, and still to reach an observer's ear placed near the origin of sound. He had also shown that *visible* clouds were quite incompetent to return any sensible echo to the loudest sounds.

So far from receding from his views in regard to the occasions of irregularity in the audibility of sound, in his last Report of the Light-House Board—that for 1877, published but a short time before his death—he announced his previous conclusions as only more confirmed by his later observations; and a summary of these conclusions was also published in the Smithsonian Report for 1877.

The ideas of sound transmission promulgated in popular books and lectures, as derived from class-room experiments, are very inaccurate and misleading when applied to any considerable range of sound travel. Were the medium of sound propagation—the atmosphere—perfectly homogeneous in density, in temperature, and in movement, the beams would indeed travel in sensibly straight lines, but still with a large amount of lateral diffusion bearing no analogy to the diffraction of light. But in distances of several miles—

say from one to ten, as involved in fog-signaling,—it may be said that such conditions of aërial uniformity are *never* present; or in other words that sound beams are never transmitted for any great distance in sensibly straight lines. And hence it is, that after every allowance for lateral deflection, there frequently remain under peculiar circumstances, intermediate points of acoustic darkness, or belts and regions of insulated silence.

The next communication was by Mr. E. B. ELLIOTT, who read from a cablegram from Berlin relative to the Monetary Conference about to meet at Paris, that a fixed legal ratio of value of gold to silver of 15½ to 1, and the unrestricted coinage of both metals at this fixed ratio of value, were to be presented to the Convention as the leading subjects for discussion, and prospective adoption.

The present market ratio is about 18 to 1, the proposed ratio 15½ to 1. Now one ounce of gold and eighteen ounces of silver are equivalents for debt-contracting and debt-paying purposes, but the proposition is that the nations enact that one ounce of gold and 15½ ounces of silver shall be legal equivalents for debt-paying purposes, the option of deciding in which of the two metals the payment shall be reckoned and paid, to be with the person making the payment, or debtor. It is a proposition then to allow the debtor to scale down his debt from 18 to 15½, to scale down his payments 14 per cent. from the existing standard;—a proposition that the nations in the payment of their public debts may diminish their payments 14 per cent. and also, that the people in their several countries may liquidate their debts, public and private at the same reduced rate, 14 per cent.

The adoption of this scheme of partial repudiation by our own or any other nation would of necessity prove disastrous to its credit.

The ability of our own country to pay its indebtedness is believed to be unsurpassed by any on the face of the globe, but its willingness is questioned, and the sending of a Commission to Europe, and inviting a conference of nations to favorably consider the subject of scaling down the value of the monetary unit of account, must tend to the depression of that credit.

If, with that doubt impending as to our *willingness* to make full payment of our indebtedness, our nation can borrow at the low rate of 3¼ or 3½ per cent. per annum; there is reason to believe that, with that doubt dispelled, our bonds can readily be placed on the world's market at the greatly improved rate of 3 per cent. per annum.

To this end it is desirable: (1), that the forced coinage of our legal tender silver dollar (of 412½ grains silver 9-10 fine) be discontinued; (2), that on all future coins and on bullion, be stamped their weight in grammes, and their fineness 9-10; and (3) that an international commission be created whose duty it shall be to periodically (annually or oftener) proclaim, based on the market quotations of the few months immediately preceding the date of the proclamation, the value in gold of an equal weight of silver; and (4) that the metric-stamped coin and bullion at the proclaimed ratio of value, shall each be equally legal tender of payment in unlimited amount, until the issuing of the next periodical proclamation.

This would be true bi-metallism. The adoption of the proposed ratio, 15½, would be silver mono-metallism under the misnomer of bi-metallism.

By the adoption of the true bi-metallic method proposed—*i. e.*, frequent periodical publication of the true market ratio, instead of a single arbitrary proclamation to last for all time—we should stand before the world with our willingness to pay undoubted, and our ability to pay unsurpassed and paramount among the nations, and our national debt could be placed on the market on more favorable terms than that of any other commercial country.

At the conclusion of Mr. Elliott's remarks, the Society adjourned.

198TH MEETING. APRIL 16, 1881.

The President in the Chair.

Fifty-four members and visitors present.

The minutes of the 196th and 197th meetings were read and adopted.

The Chair announced to the Society the election to membership of Mr. WILLIAM A. DECAINDRY.

The first communication of the evening was by Mr. ALEXANDER GRAHAM BELL, announcing to the Society, the discovery of

THE SPECTROPHONE.

In a paper read before the American Association for the Advancement of science, last August, I described certain experiments made by Mr. Sumner Tainter and myself, which had resulted in the construction of a "*Photophone*," or apparatus for the production of sound by light;* and it will be my object to-day to describe the progress we have made in the investigation of photophonic phenomena since the date of this communication.

In my Boston paper the discovery was announced, that thin disks of very many different substances *emitted sounds* when exposed to the action of a rapidly-interrupted beam of sunlight. The great variety of material used in these experiments led me to believe that sonorousness under such circumstances would be found to be a general property of all matter.

At that time we had failed to obtain audible effects from masses of the various substances which became sonorous in the condition of thin diaphragms, but this failure was explained upon the supposition that the molecular disturbance produced by the light was chiefly a surface action, and that under the circumstances of the experiments, the vibration had to be transmitted through the mass of the substance in order to affect the ear. It was therefore supposed that, if we could lead to the ear, air that was directly in contact with the illuminated surface, louder sounds might be obtained, and solid masses be found to be as sonorous as thin diaphragms. First experiments made to verify this hypothesis pointed towards success. A beam of sunlight was focussed into one end of an open tube, the ear being placed at the other end. Upon interrupting the beam, a clear, musical tone was heard, the pitch depending upon the frequency of the interruption of the light, and the loudness upon the material composing the tube.

At this stage our experiments were interrupted, as circumstances called me to Europe.

While in Paris a new form of the experiment occurred to my mind, which would not only enable us to investigate the sounds

* Proceedings of American Association for the Advancément of Science, Aug. 27th, 1880; see, also, American Journal of Science, vol. xx, p. 305; Journal of the American Electrical Society, vol. iii, p. 3; Journal of the Society of Telegraph Engineers and Electricians, vol. ix, p. 404; Annales de Chimie et de Physique, vol. xxi.

produced by masses, but would also permit us to test the more general proposition that *sonorousness, under the influence of intermittent light, is a property common to all matter.*

The substance to be tested was to be placed in the interior of a transparent vessel made of some material, which (like glass) is transparent to light, but practically opaque to sound.

Under such circumstances the light could get in, but the sound produced by the vibration of the substance could not get out. The audible effects could be studied by placing the ear in communication with the interior of the vessel by means of a hearing tube.

Some preliminary experiments were made in Paris to test this idea, and the results were so promising that they were communicated to the French Academy on the 11th of October, 1880, in a note read for me by Mr. Antoine Breguet.* Shortly afterwards I wrote to Mr. Tainter, suggesting that he should carry on the investigation in America, as circumstances prevented me from doing so myself in Europe. As these experiments seemed to have formed the common starting point for a series of independent researches of the most important character carried on simultaneously in America by Mr. Tainter, and in Europe by M. Mercadier,† Prof. Tyndall,‡ W. E. Rönton,§ and W. H. Preece,‖ I may be permitted to quote from my letter to Mr. Tainter the passage describing the experiments referred to:

"METROPOLITAN HOTEL, RUE CAMBON, PARIS,
"*Nov.* 2, 1880.

"DEAR MR. TAINTER: * * * I have devised a method of producing sounds by the action of an intermittent beam of light from substances that cannot be obtained in the shape of thin diaphragms or in the tubular form; indeed, the method is specially adapted to testing the generality of the phenomenon we have discovered, as it can be adapted to solids, liquids, and gases.

"Place the substance to be experimented with in a glass test-tube,

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* *Comptes Rendus*, vol. xcl, p. 595.

† "Notes on Radiophony," *Comptes Rendus*, Dec. 6 and 13, 1880; Feb. 21 and 28, 1881. See, also, *Journal de Physique*, vol. x, p. 53.

‡ "Action of an Intermittent Beam of Radiant Heat upon Gaseous Matter." *Proc. Royal Society*, Jan. 13, 1881, vol. xxxi, p. 307.

§ "On the tones which arise from the intermittent illumination of a gas." See *Annalen der Phys. und Chemie*, Jan., 1881, No. 1, p. 155.

‖ "On the conversion of Radiant Energy into Sonorous Vibration." *Proc. Royal Society*, March 10, 1881, vol. xxxi, p. 506.

connect a rubber tube with the mouth of the test-tube, placing the other end of the pipe to the ear. Then focus the intermittent beam upon the substance in the tube. I have tried a large number of substances in this way with great success, although it is extremely difficult to get a glimpse of the sun here, and when it does shine the intensity of the light is not to be compared with that to be obtained in Washington. I got splendid effects from crystals of bichromate of potash, crystals of sulphate of copper, and from tobacco smoke. A whole cigar placed in the test-tube produced a very loud sound. I could not hear anything from plain water, but when the water was discolored with ink a feeble sound was heard. I would suggest that you might repeat these experiments and extend the results," &c., &c.

Upon my return to Washington in the early part of January.* Mr. Tainter communicated to me the results of the experiments he had made in my laboratory during my absence in Europe.

He had commenced by examining the sonorous properties of a vast number of substances enclosed in test-tubes in a simple empirical search for loud effects. He was thus led gradually to the discovery that cotton-wool, worsted, silk, and fibrous materials generally, produced much louder sounds than hard rigid bodies like crystals, or diaphragms such as we had hitherto used.

In order to study the effects under better circumstances he enclosed his materials in a conical cavity in a piece of brass, closed by a flat plate of glass. A brass tube leading into the cavity served for connection with the hearing-tube. When this conical cavity was stuffed with worsted or other fibrous materials the sounds produced were much louder than when a test-tube was employed. This form of receiver is shown in Figure I.

Mr. Tainter next collected silks and worsteds of different colors, and speedily found that the darkest shades produced the best effects. Black worsted especially gave an extremely loud sound.

As white cotton wool had proved itself equal, if not superior, to any other white fibrous material before tried, he was anxious to obtain colored specimens for comparison. Not having any at hand, however, he tried the effect of darkening some cotton-wool with lamp-black. Such a marked reinforcement of the sound resulted that he was induced to try lamp-black alone.

About a teaspoonful of lamp-black was placed in a test-tube and

* On the 7th of January.

exposed to an intermittent beam of sunlight. The sound produced was much louder than any heard before.

Upon smoking a piece of plate-glass, and holding it in the intermittent beam with the lamp-black surface towards the sun, the sound produced was loud enough to be heard, with attention, in any part of the room. With the lamp-black surface turned from the sun the sound was much feebler.

Mr. Tainter repeated these experiments for me immediately upon my return to Washington, so that I might verify his results.

Upon smoking the interior of the conical cavity shown in Figure I, and then exposing it to the intermittent beam, with the glass lid in position as shown, the effect was perfectly startling. The sound was so loud as to be actually painful to an ear placed closely against the end of the hearing-tube.

The sounds, however, were sensibly louder when we placed some smoked wire gauze in the receiver, as illustrated in the drawing, Figure I.

When the beam was thrown into a resonator, the interior of which had been smoked over a lamp, most curious alternations of sound and silence were observed. The interrupting disk was set rotating at a high rate of speed, and was then allowed to come gradually to rest. An extremely feeble musical tone was at first heard, which gradually fell in pitch as the rate of interruption grew less. The loudness of the sound produced varied in the most interesting manner. Minor reinforcements were constantly occurring, which became more and more marked as the true pitch of the resonator was neared. When at last the frequency of interruption corresponded to the frequency of the fundamental of the resonator, the sound produced was so loud that it might have been heard by an audience of hundreds of people.

The effects produced by lamp-black seemed to me to be very extraordinary, especially as I had a distinct recollection of experiments made in the summer of 1880 with smoked diaphragms, in which no such reinforcement was noticed.

Upon examining the records of our past photophonic experiments we found in vol. vii, p. 57, the following note:

"Experiment V.—Mica diaphragm covered with lamp-black on side exposed to light.

"Result: distinct sound about same as without lamp-black.—*A. G. B.*, *July* 18*th*, 1880.

"Verified the above, but think it somewhat louder than when used without lamp-black."—*S. T., July 18th,* 1880.

Upon repeating this old experiment we arrived at the same result as that noted. Little if any augmentation of sound resulted from smoking the mica. In this experiment the effect was observed by placing the mica diaphragm against the ear, and also by listening through a hearing-tube, one end of which was closed by the diaphragm. The sound was found to be more audible through the free air when the ear was placed as near to the lamp-black surface as it could be brought without shading it.

At the time of my communication to the American Association I had been unable to satisfy myself that the substances which had become sonorous under the direct influence of intermittent sunlight were capable of reproducing sounds of articulate speech under the action of an undulatory beam from our photophonic transmitter. The difficulty in ascertaining this will be understood by considering that the sounds emitted by thin diaphragms and tubes were so feeble that it was impracticable to produce audible effects from substances in these conditions at any considerable distance away from the transmitter; but it was equally impossible to judge of the effects produced by our articulate transmitter at a short distance away, because the speaker's voice was directly audible through the air. The extremely loud sounds produced from lamp-black have enabled us to demonstrate the feasibility of using this substance in an articulating photophone in place of the electrical receiver formerly employed.

The drawing (Fig. 2) illustrates the mode in which the experiment was conducted. The diaphragm of the transmitter (A) was only 5 centimeters in diameter, the diameter of the receiver (B) was also 5 centimeters, and the distance between the two was 40 meters, or 800 times the diameter of the transmitter diaphragm. We were unable to experiment at greater distances without a heliostat on account of the difficulty of keeping the light steadily directed on the receiver. Words and sentences spoken into the transmitter in a low tone of voice were audibly reproduced by the lamp-black receiver.

In Fig. 3 is shown a mode of interrupting a beam of sunlight for producing distant effects without the use of lenses. Two similarly-perforated disks are employed, one of which is set in rapid rotation, while the other remains stationary. This form of interrupter is also admirably adapted for work with artificial light. The receiver illustrated in the drawing consists of a parabolic reflector, in the focus of which is placed a glass vessel (A) containing lamp-black, or other sensitive substance, and connected with a hearing-tube. The beam of light is interrupted by its passage through the two slotted disks shown at B, and in operating the instrument musical signals like the dots and dashes of the Morse alphabet are produced from the sensitive receiver (A) by slight motions of the mirror(C) about its axis (D.)

In place of the parabolic reflector shown in the figure a conical reflector like that recommended by Prof. Sylvanus Thompson * can be used, in which case a cylindrical glass vessel would be preferable to the flask (A) shown in the figure.

In regard to the sensitive materials that can be employed, our experiments indicate that in the case of solids the physical condition and the color are two conditions that markedly influence the intensity of the sonorous effects. *The loudest sounds are produced from substances in a loose, porous, spongy condition, and from those that have the darkest or most absorbent colors.*

The materials from which the best effects have been produced are cotton-wool, worsted, fibrous materials generally, cork, sponge, platinum and other metals in a spongy condition, and lamp-black.

The loud sounds produced from such substances may perhaps be explained in the following manner: Let us consider, for example, the case of lamp-black—a substance which becomes heated by exposure to rays of all refrangibility. I look upon a mass of this substance as a sort of sponge, with its pores filled with air instead of water. When a beam of sunlight falls upon this mass, the particles of lamp-black are heated, and consequently expand, causing a contraction of the air-spaces or pores among them.

Under these circumstances a pulse of air should be expelled, just as we would squeeze out water from a sponge.

The force with which the air is expelled must be greatly increased by the expansion of the air itself, due to contact with the heated particles of lamp-black. When the light is cut off the converse process takes place. The lamp-black particles cool and contract, thus enlarging the air spaces among them, and the enclosed air also becomes cool. Under these circumstances a partial vacuum should

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* Phil. Mag., April, 1881, vol. xi, p. 286.

be formed among the particles, and the outside air would then be absorbed, as water is by a sponge when the pressure of the hand is removed.

I imagine that in some such manner as this a wave of condensation is started in the atmosphere each time a beam of sunlight falls upon lamp-black, and a wave of rarefaction is originated when the light is cut off. *We can thus understand how it is that a substance like lamp-black produces intense sonorous vibrations in the surrounding air, while at the same time it communicates a very feeble vibration to the diaphragm or solid bed upon which it rests.*

This curious fact was independently observed in England by Mr. Preece, and it led him to question whether, in our experiments with thin diaphragms, the sound heard was due to the vibration of the disk or (as Prof. Hughes had suggested) to the expansion and contraction of the air in contact with the disk confined in the cavity behind the diaphragm. In his paper read before the Royal Society on the 10th of March, Mr. Preece describes experiments from which he claims to have proved that the effects are wholly due to the vibrations of the confined air, and that the *disks do not vibrate at all.*

I shall briefly state my reasons for disagreeing with him in this conclusion:

1. When an intermittent beam of sunlight is focussed upon a sheet of hard rubber or other material, a musical tone can be heard, not only by placing the ear immediately behind the part receiving the beam, but by placing it against any portion of the sheet, even though this may be a foot or more from the place acted upon by the light.

2. When the beam is thrown upon the diaphragm of a "Blake Transmitter," a loud musical tone is procuced by a telephone connected in the same galvanic circuit with the carbon button, (A,) Fig. 4. Good effects are also produced when the carbon button (A) forms, with the battery, (B,) a portion of the primary circuit of an induction coil, the telephone (C) being placed in the secondary circuit.

In these cases the wooden box and mouth-piece of the transmitter should be removed, so that no air-cavities may be left on either side of the diaphragm.

It is evident, therefore, that in the case of thin disks a real vibration of the diaphragm is caused by the action of the intermittent beam, in-

dependently of any expansion and contraction of the air confined in the cavity behind the diaphragm.

Lord Rayleigh has shown mathematically that a two-and-fro vibration of sufficient amplitude to produce an audible sound would result from a periodical communication and abstraction of heat, and he says: "We may conclude, I think, that there is at present no reason for discarding the obvious explanation that the sounds in question are due to the bending of the plates under unequal heating." (Nature, xxiii, p. 274.) Mr. Preece, however, seeks to prove that the sonorous effects cannot be explained upon this supposition; but his experimental proof is inadequate to support his conclusion. Mr. Preece expected that if Lord Rayleigh's explanation was correct, the expansion and contraction of a thin strip under the influence of an intermittent beam could be caused to open and close a galvanic circuit, so as to produce a musical tone from a telephone in the circuit. But this was an inadequate way to test the point at issue, for Lord Rayleigh has shown (Proc. of Roy. Soc., 1877,) that an audible sound can be produced by a vibration, whose amplitude is *less than a ten-millionth of a centimetre*, and certainly such a vibration as that would not have sufficed to operate a "make-and-break contact" like that used by Mr. Preece. The negative results obtained by him cannot, therefore, be considered conclusive.

The following experiments (devised by Mr. Tainter) have given results decidedly more favorable to the theory of Lord Rayleigh than to that of Mr. Preece:

1. A strip (A) similar to that used in Mr. Preece's experiment was attached firmly to the centre of an iron diaphragm, (B,) as shown in Figure 5, and was then pulled taut at right angles to the plane of the diaphragm. When the intermittent beam was focussed upon the strip (A) a clear musical tone could be heard by applying the ear to the hearing tube (C,)

This seemed to indicate a rapid expansion and contraction of the substance under trial.

But a vibration of the diaphragm (B) would also have resulted if the thin strip (A) had acquired a to-and-fro motion, due either to the direct impact of the beam or to the sudden expansion of the air in contact with the strip.

2. To test whether this had been the case an additional strip (D)

was attached by its central point only to the strip under trial, and was then submitted to the action of the beam, as shown in Fig. 6.

It was presumed that if the vibration of the diaphragm (B) had been due to a *pushing force* acting on the strip (A,) the addition of the strip (D) would not interfere with the effect. But if, on the other hand, it had been due to the longitudinal expansion and contraction of the strip, (A,) the sound would cease, or, at least, be reduced. The beam of light falling upon strip (D) was now interrupted as before by the rapid rotation of a perforated disk, which was allowed to come gradually to rest.

No sound was heard excepting at a certain speed of rotation, when a feeble musical tone became audible.

This result is confirmatory of the first.

The audibility of the effect at a particular rate of interruption suggests the explanation that the strip (D) had a normal rate of vibration of its own.

When the frequency of the interruption of the light corresponded to this, the strip was probably thrown into vibration after the manner of a tuning fork, in which case a to-and-fro vibration would be propagated down its stem or central support to the strip (A.)

This indirectly proves the value of the experiment.

The list of solid substances that have been submitted to experiment in my laboratory is too long to be quoted here, and I shall merely say that we have not yet found one solid body that has failed to become sonorous under proper conditions of experiment.*

Experiments with Liquids.

The sounds produced by liquids are much more difficult to observe than those produced by solids. The high absortive power possessed by most liquids would lead one to expect intense vibrations from the action of intermittent light, but the number of sonorous liquids that have so far been found is extremely limited, and the sounds produced are so feeble as to be heard only by the greatest attention and under the best circumstances of experiment.

* Carbon and thin microscopic glass are mentioned in my Boston paper as non-responsive, and powdered chlorate of potash in the communication to the French Academy, (Comtes Rendus, vol. xcl, p. 595.) All these substances have since yielded sounds under more careful conditions of experiment.

In the experiments made in my laboratory a very long test-tube was filled with the liquid under examination, and a flexible rubber-tube was slipped over the mouth far enough down to prevent the possibility of any light reaching the vapor above the surface. Precautions were also taken to prevent reflection from the bottom of the test-tube. An intermittent beam of sunlight was then focussed upon the liquid in the middle portion of the test-tube by means of a lens of large diameter.

Results.

Clear water	No sound audible.
Water discolored by ink	Feeble sound.
Mercury	No sound heard.
Sulphuric ether *	Feeble, but distinct sound.
Ammonia	" "
Ammonia-sulphate of copper	" "
Writing ink	" "
Indigo in sulphuric acid	" "
Chloride of copper *	" "

The liquids distinguished by an asterisk gave the best sounds.

Acoustic vibrations are always much enfeebled in passing from liquids to gases, and it is probable that a form of experiment may be devised which will yield better results by communicating the vibrations of the liquid to the ear through the medium of a solid rod.

Experiments with Gaseous Matter.

On the 29th of November, 1880, I had the pleasure of showing to Prof. Tyndall, in the laboratory of the Royal Institution, the experiments described in the letter to Mr. Tainter from which I have quoted above, and Prof. Tyndall at once expressed the opinion that the sounds were due to rapid changes of temperature in the body submitted to the action of the beam. Finding that no experiments had been made at that time to test the sonorous properties of different gases, he suggested filling one test-tube with the vapor of sulphuric ether, (a good absorbent of heat,) and another with the vapor of bi-sulphide of carbon, (a poor absorbent,) and he predicted that if any sound was heard it would be louder in the former case than in the latter.

The experiment was immediately made, and the result verified the prediction.

Since the publication of the memoirs of Röntgen* and Tyndall† we have repeated these experiments, and have extended the inquiry to a number of other gaseous bodies, obtaining in every case similar results to those noted in the memoirs referred to.

The vapors of the following substances were found to be highly sonorous in the intermittent beam: Water vapor, coal gas, sulphuric ether, alchohol, ammonia, amylene, ethyl bromide, diethylamene, mercury, iodine, and peroxide of nitrogen. The loudest sounds were obtained from iodine and peroxide of nitrogen.

I have now shown that sounds are produced by the direct action of intermittent sunlight from substances in every physical condition, (solids, liquid, and gaseous,) and the probability is therefore very greatly increased that sonorousness under such circumstances will be found to be a universal property of matter.

Upon Substitutes for Selenium in Electrical Receivers.

At the time of my communication to the American Association the loudest effects obtained were produced by the use of selenium, arranged in a cell of suitable construction, and placed in a galvanic circuit with a telephone. Upon allowing an intermittent beam of sunlight to fall upon the selenium a musical tone of great intensity was produced from the telephone connected with it.

But the selenium was very inconstant in its action. It was rarely, if ever, found to be the case, that two pieces of selenium (even of the same stick) yielded the same results under identical circumstances of annealing, &c. While in Europe last autumn, Dr. Chichester Bell, of University College, London, suggested to me that this inconstancy of result might be due to chemical impurities in the selenium used. Dr. Bell has since visited my laboratory in Washington, and has made a chemical examination of the various samples of selenium I had collected from different parts of the world. As I understand it to be his intention to publish the results of this analysis very soon, I shall make no further mention of his investigation than to state that he has found sulphur, iron, lead, and arsenic in the so-called "selenium," with traces of organic matter; that a quantitative examination has revealed the fact that sulphur constitutes nearly one per cent. of the whole mass; and that when

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* Ann. der Phys. und Chem., 1881, No. 1, p. 155.

† Proc. Roy. Soc., vol. xxxi, p. 307.

these impurities are eliminated the selenium appears to be more constant in its action and more sensitive to light.

Prof. W. G. Adams* has shown that tellurium, like selenium, has its electrical resistance affected by light, and we have attempted to utilize this substance in place of selenium. The arrangement of cell (shown in Fig. 7) was constructed for this purpose in the early part of 1880; but we failed at that time to obtain any indications of sensitiveness with a reflecting galvanometer. We have since found, however, that when this tellurium spiral is connected in circuit with a galvanic battery and telephone, and exposed to the action of an intermittent beam of sunlight, a distinct musical tone is produced by the telephone. The audible effect is much increased by placing the tellurium cell with the battery in the primary circuit of an induction coil, and placing the telephone in the secondary circuit.

The enormously high resistance of selenium and the extremely low resistance of tellurium suggested the thought that an alloy of these two substances might possess intermediate electrical properties. We have accordingly mixed together selenium and tellurium in different proportions, and, while we do not feel warranted at the present time in making definite statements concerning the results, I may say that such alloys have proved to be sensitive to the action of light.

It occurred to Mr. Tainter before my return to Washington last January, that the very great molecular disturbance produced in lamp-black by the action of the intermittent sunlight should produce a corresponding disturbance in an electric current passed through it, in which case lamp-black could be employed in place of selenium in an electrical receiver. This has turned out to be the case, and the importance of the discovery is very great, especially when we consider the expense of such rare substances as selenium and tellurium.

The form of lamp-black cell we have found most effective is shown in Fig. 8. Silver is deposited upon a plate of glass, and a zigzag line is then scratched through the film, as shown, dividing the silver surface into two portions insulated from one another, having the form of two combs with interlocking teeth.

Each comb is attached to a screw-cup, so that the cell can be

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* Proc. Roy. Soc., vol. xxiv, p. 163.

placed in an electrical circuit when required. The surface is then smoked until a good film of lamp-black is obtained, filling the interstices between the teeth of the silver combs. When the lamp-black cell is connected with a telephone and galvanic battery, and exposed to the influence of an intermittent beam of sunlight, a loud musical tone is produced by the telephone. This result seems to be due rather to the physical condition than to the nature of the conducting material employed, as metals in a spongy condition produce similar effects. For instance, when an electrical current is passed through spongy platinum, while it is exposed to intermittent sunlight, a distinct musical tone is produced by a telephone in the same circuit. In all such cases the effect is increased by the use of an induction coil; and the sensitive cells can be employed for the reproduction of an articulate speech as well as for the production of musical sounds.

We have also found that loud sounds are produced from lamp-black by passing through it an intermittent electrical current; and that it can be used as a telephonic receiver for the reproduction of articulate speech by electrical means.

A convenient mode of arranging a lamp-black cell for experimental purposes is shown in Fig. 9. When an intermittent current is passed through the lamp-black, (A,) or when an intermittent beam of sunlight falls upon it through the glass plate B, a loud musical tone can be heard by applying the ear to the hearing-tube C. When the light and the electrical current act simultaneously, two musical tones are perceived, which produce beats when nearly of the same pitch. By proper arrangements a complete interference of sound can undoubtedly be produced.

Upon the Measurement of the Sonorous Effects produced by Different Substances.

We have observed that different substances produce sounds of very different intensities under similar circumstances of experiment, and it has appeared to us that very valuable information might be obtained if we could measure the audible effects produced. For this purpose we have constructed several different forms of apparatus for studying the effects, but as our researches are not yet complete, I shall confine myself to a simple description of some of the forms of apparatus we have devised.

When a beam of light is brought to a focus by means of a lens, the beam diverging from the focal point becomes weaker as the distance increases in a calculable degree. Hence, if we can determine the distances from the focal point at which two different substances emit sounds of equal intensity, we can calculate their relative sonorous powers.

Preliminary experiments were made by Mr. Tainter during my absence in Europe to ascertain the distance from the focal point of a lens at which the sound produced by a substance became inaudible. A few of the results obtained will show the enormous differences existing between the different substances in this respect.

Distance from Focal Point of Lens at which Sounds became Inaudible with Different Substances.

Zinc diaphragm, (polished)		1.51 m.
Hard rubber diaphragm		1.90 m.
Tin-foil "		2.00 m.
Telephone "	(Japanned iron)	2.15 m.
Zinc "	(unpolished)	2.15 m.
White silk,	(In receiver shown in Fig. 1.)	3.10 m.
White worsted,	" " "	4.01 m.
Yellow worsted,	" " "	4.06 m.
Yellow silk,	" " "	4.13 m.
White cotton-wool,	" " "	4.38 m.
Green silk,	" " "	4.52 m.
Blue worsted,	" " "	4.69 m.
Purple silk,	" " "	4.82 m.
Brown silk,	" " "	5.02 m.
Black silk,	" " "	5.21 m.
Red silk,	" " "	5.24 m.
Black worsted,	" " "	6.50 m.
Lamp-black. In this case the limit of audibility could not be determined on account of want of space. Sound perfectly audible at a distance of		10.00 m.

Mr. Tainter was convinced from these experiments that this field of research promised valuable results, and he at once devised an apparatus for studying the effects, which he described to me upon my return from Europe. The apparatus has since been constructed and I take great pleasure in showing it to you to-day.

(1.) A beam of light is received by two similar lenses, (A B, Fig. 10,) which brings the light to a focus on either side of the interrupting disk (C.) The two substances, whose sonorous powers are to be compared, are placed in the receiving vessels (D E) (so arranged as to expose equal surfaces to the action of the beam) which communicate by flexible tubes (F G) of equal length, with the common hearing-tube (H.) The receivers (D E) are placed upon slides, which can be moved along the graduated supports (I K.) The beams of light passing through the interrupting disk (C) are alternately cut off by the swinging of a pendulum, (L.) Thus a musical tone is produced alternately from the substance in D and from that in E. One of the receivers is kept at a constant point upon its scale, and the other receiver is moved towards or from the focus of its beam until the ear decides that the sounds produced from D and E are of equal intensity. The relative positions of the receivers are then noted.

(2.) Another method of investigation is based upon the production of an interference of sound, and the apparatus employed is shown in Fig. 11. The interrupter consists of a tuning-fork, (A,) which is kept in continuous vibration by means of an electro-magnet, (B.)

A powerful beam of light is brought to a focus between the prongs of the tuning-fork, (A,) and the passage of the beam is more or less obstructed by the vibration of the opaque screens (C D) carried by the prongs of the fork.

As the tuning-fork (A) produces a sound by its own vibration, it is placed at a sufficient distance away to be inaudible through the air, and a system of lenses is employed for the purpose of bringing the undulating beam of light to the receiving lens (E) with as little loss as possible. The two receivers (F G) are attached to slides (H I) which move upon opposite sides of the axis of the beam, and the receivers are connected by flexible tubes of unequal length (K L) communicating with the common hearing-tube (M.)

The length of the tube (K) is such that the sonorous vibrations from the receivers (F G) reach the common hearing-tube (M) in opposite phases. Under these circumstances silence is produced when the vibrations in the receivers (F G) are of equal intensity. When the intensities are unequal, a residual effect is perceived. In operating the instrument the position of the receiver (G) remains constant, and the receiver (F) is moved to or from the focus of the beam until complete silence is produced. The relative positions of the two receivers are then noted.

(3.) Another mode is as follows: The loudness of a musical tone produced by the action of light is compared with the loudness of a tone of similar pitch produced by electrical means. A rheostat introduced into the circuit enables us to measure the amount of resistance required to render the electrical sound equal in intensity to the other.

(4.) If the tuning-fork (A) in Fig. 11 is thrown into vibration by an undulatory instead of an intermittent current passed through the electro-magnet, (B,) it is probable that a musical tone, electrically produced in the receiver (F) by the action of the same current, would be found capable of extinguishing the effect produced in the receiver (G) by the action of the undulatory beam of light, in which case it should be possible to establish an acoustic balance between the effects produced by light and electricity by introducing sufficient resistance into the electric circuit.

Upon the Nature of the Rays that Produce Sonorous Effects in Different Substances.

In my paper read before the American Association last August and in the present paper I have used the word "light" in its usual rather than its scientific sense, and I have not hitherto attempted to discriminate the effects produced by the different constituents of ordinary light, the thermal, luminous, and actinic rays. I find, however, that the adoption of the word "photophone" by Mr. Tainter and myself has led to the assumption that we belived the audible effects discovered by us to be due entirely to the action of luminous rays. The meaning we have uniformly attached to the words "photophone" and "light" will be obvious from the following passage, quoted from my Boston paper:

"Although effects are produced as above shown by forms of radiant energy, which are invisible, we have named the apparatus for the production and reproduction of sound in this way the 'photophone' *because an ordinary beam of light contains the rays which are operative.*"

To avoid in future any misunderstanding upon this point we have decided to adopt the term "*radiophone,*" proposed by Mr. Mercadier, as a general term signifying an apparatus for the production of sound by any form of radiant energy, limiting the words *thermophone, photophone,* and *actinophone* to apparatus for the production of sound by thermal, luminous, or actinic rays respectively.

M. Mercadier, in the course of his researches in radiophony, passed an intermittent beam from an electric lamp through a prism, and then examined the audible effects produced in different parts of the spectrum. (*Comptes Rendus*, Dec. 6th, 1880.)

We have repeated this experiment, using the sun as our source of radiation, and have obtained results somewhat different from those noted by M. Mercadier.

(1.) A beam of sunlight was reflected from a heliostat (A, Fig. 12) through an achromatic lens, (B,) so as to form an image of the sun upon the slit (C.)

The beam then passed through another achromatic lens (D) and through a bisulphide of carbon prism, (E,) forming a spectrum of great intensity, which, when focused upon a screen, was found to be sufficiently pure to show the principal absorption lines of the solar spectrum.

The disk interrupter (F) was then turned with sufficient rapidity to produce from five to six hundred interruptions of the light per second, and the spectrum was explored with the receiver, (G,) which was so arranged that the lamp-black surface exposed was limited by a slit, as shown.

Under these circumstances sounds were obtained in every part of the visible spectrum, excepting the extreme half of the violet, as well as in the ultra-red. A continuous increase in the loudness of the sound was observed upon moving the receiver (G) gradually from the violet into the ultra-red. The point of maximum sound lay very far out in the ultra-red. Beyond this point the sound began to increase, and then stopped so suddenly that a very slight motion of the receiver (G) made all the difference between almost maximum sound and complete silence.*

(2.) The lamp-blacked wire gauze was then removed and the interior of the receiver (G) was filled with red worsted. Upon exploring the spectrum as before, entirely different results were obtained. The maximum effect was produced in the green at that part where the red worsted appeared to be black. On either side of this point the sound gradually died away, becoming inaudible on the one side in the middle of the indigo, and on the other at a short distance outside the edge of the red.

*The results obtained in this and subsequent experiments are shown in a tabulated form in Fig. 14.

(3.) Upon substituting green silk for red worsted, the limits of audition appeared to be the middle of the blue and a point a short distance out in the ultra-red. Maximum in the red.

(4.) Some hard-rubber shavings were now placed in the receiver (G.) The limits of audibility appeared to be on the one hand the junction of the green and blue, and on the other the outside edge of the red. Maximum in the yellow. Mr. Tainter thought he could hear a little way into the ultra-red, and to his ear the maximum was about the junction of the red and orange.

(5.) A test-tube containing the vapor of sulphuric ether was then substituted for the receiver (G.) Commencing at the violent end the test-tube was gradually moved down the spectrum and out into the ultra-red without audible effect, but when a certain point far out in the ultra-red was reached, a distinct musical tone suddenly made its appearance, which disappeared as suddenly on moving the test-tube a very little further on.

(6.) Upon exploring the spectrum with a test-tube containing the vapor of iodine, the limits of audibility appeared to be the middle of the red and the junction of the blue and indigo. Maximum in the green.

(7.) A test-tube containing peroxide of nitrogen was substituted for that containing iodine. Distinct sounds were obtained in all parts of the visible spectrum, but no sounds were observed in the ultra-red. The maximum effect seemed to me to be in the blue. The sounds were well marked in all parts of the violet, and I even fancied that the audible effect extended a little way into the ultra-violet, but of this I cannot be certain. Upon examining the absorption spectrum of peroxide of nitrogen it was at once observed that the maximum sound was produced in that part of the spectrum where the greatest number of absorption lines made their appearance.

(8.) The spectrum was now explored by a selenium cell, and the audible effects were observed by means of a telephone in the same galvanic circuit with the cell. The maximum effect was produced in the red about its junction with the orange. The audible effect extended a little way into the ultra-red on the one hand and up as high as the middle of the violet on the other.

Although the experiments so far made can only be considered as preliminary to others of a more refined nature, I think we are warranted in concluding that *the nature of the rays that produce sonorous effects in different substances depends upon the nature of the* *substances that are exposed to the beam, and that the sounds are in every case due to those rays of the spectrum that are absorbed by the body.*

The Spectrophone.

Our experiments upon the range of audibility of different substances in the spectrum have led us to the construction of a new instrument for use in spectrum analysis. The eye-piece of a spectroscope is removed, and sensitive substances are placed in the focal point of the instrument behind an opaque diaphragm containing a slit. These substances are put in communication with the ear by means of a hearing-tube, and thus the instrument is converted into a veritable "spectrophone," like that shown in Fig. 13.

Suppose we smoke the interior of our spectrophone receiver, and fill the cavity with peroxide of nitrogen gas. We have then a combination that gives us good sounds in all parts of the spectrum, (visible and invisible,) except the ultra-violet. Now, pass a rapidly-interrupted beam of light through some substance whose absorption spectrum is to be investigated, and bands of sound and silence are observed upon exploring the spectrum, the silent positions corresponding to the absorption bands. Of course, the ear cannot for one moment compete with the eye in the examination of the visible part of the spectrum; but in the invisible part beyond the red, where the eye is useless, the ear is invaluable. In working in this region of the spectrum, lamp-black alone may be used in the spectrophonic receiver. Indeed, the sounds produced by this substance in the ultra-red are so well marked as to constitute our instrument a most reliable and convenient substitute for the thermo-pile. A few experiments that have been made may be interesting.

(1.) The interrupted beam was filtered through a saturated solution of alum.

Result: The range of audibility in the ultra-red was slightly reduced by the absorption of a narrow band of the rays of lowest refrangibility. The sounds in the visible part of the spectrum seemed to be unaffected.

(2.) A thin sheet of hard rubber was interposed in the path of the beam.

Result: Well-marked sounds in every part of the ultra-red. No sounds in the visible part of the spectrum, excepting the extreme half of the red.

These experiments reveal the cause of the curious fact alluded to in my paper read before the American Association last August—that sounds were heard from selenium when the beam was filtered through both hard rubber and alum at the same time. (See table of results in Fig. 14.)

(3.) A solution of ammonia-sulphate of copper was tried.

Result: When placed in the path of the beam the spectrum disappeared, with the exception of the blue and violet end. To the eye the spectrum was thus reduced to a single broad band of blue-violet light. To the ear, however, the spectrum revealed itself as two bands of sound with a broad space of silence between. The invisible rays transmitted constituted a narrow band just outside the red.

I think I have said enough to convince you of the value of this new method of examination, but I do not wish you to understand that we look upon our results as by any means complete. It is often more interesting to observe the first totterings of a child than to watch the firm tread of a full-grown man, and I feel that *our* first footsteps in this new field of science may have more of interest to you than the fuller results of mature research. This must be my excuse for having dwelt so long upon the details of incomplete experiments.

I recognize the fact that the spectrophone must ever remain a mere adjunct to the spectroscope, but I anticipate that it has a wide and independent field of usefulness in the investigation of absorption spectra in the ultra-red.

Mr. Wm. B. Taylor inquired whether the sounds obtained from the two absorpion bands of the ammonia-sulphate of copper were octaves of each other. Mr. Bell replied that this matter had not as yet been investigated.

Prof. William B. Rogers, President of the National Academy of Sciences, being present as an invited guest, paid a high tribute to Mr. Bell upon the very great interest and high scientific value of the discovery just announced.

The next communication was by Mr. G. Brown Goode on the

SWORD-FISH AND ITS ALLIES.

This paper will be found published in full in the Annual Report of the United States Fish Commission for the year 1880.

At the conclusion of Mr. GOODE's paper the Society adjourned.

199TH MEETING. APRIL 30, 1881.

The President in the chair.

Forty-eight members present.

The recorder of the minutes of the last meeting being absent their consideration was postponed.

Mr. W. H. DALL made a communication on

RECENT DISCOVERIES IN ALASKA NORTH OF BEHRING STRAIT,

in which he alluded to the investigations carried on by the U. S. R. S. Corwin, Capt. Hooper, during the summer of 1880, including meteorology, sea temperatures and currents, as well as the investigation of the coal mines near Cape Lisburne. He described some observations made by the U. S. Coast Survey party under his charge in the same region and season, on board the U. S. S. Yukon. The migration of the Asiatic Eskimo; the sources of the warm waters of the eastern half of Behring Strait in Kotzebue and Norton Sound waters, moved by the tidal and river flow; the existence of a supposed new species of sheep allied to the Rocky Mountain bighorn (*Ovis montana*) in the east Siberian peninsula, and the character of Arctic vegetations were spoken of. Reasons for doubting the truth of the account of an alleged landing on Wrangell Land, in 1866, described in the Bremen Geographical Society's publication by a Capt. Dallmann were brought forward, and it was pointed out that the existence of Plover Island, of Siberian musk-oxen, and of certain conditions of the ice alleged by Dallmann, were in conflict with all that is definitely known by scientific men of those matters.

Remarks upon this paper were made by Messrs. ANTISELL, WHITE, FARQUHAR, HARKNESS, ALVORD, MASON, HAZEN, WELLING, ABBE, BESSELS, and GILL.

Mr. J. S. BILLINGS commenced a paper on Mortality Statistics of the Tenth Census, but at the usual hour of adjournment it was interrupted, to be resumed at the following meeting.

The Society then adjourned.

200TH MEETING. MAY 14, 1881.

The President in the Chair.

Thirty-six members present.

The minutes of the last two meetings were read and adopted.

The first communication of the evening was the continuation by Mr. J. S. BILLINGS of his remarks upon

MORTALITY STATISTICS OF THE TENTH CENSUS.

[Abstract.]

Mr. J. S. BILLINGS described the methods used in the Tenth Census to secure completeness and accuracy in the returns of mortality. The Superintendent of the Census sought to secure the aid of the physicians of the country, and for this purpose sent to each a small blank book, each leaf of which was arranged to record the facts connected with a single death. 70,306 such books were issued, and 24,057 returned at the end of the census year. The data from these books were compiled by causes of death, age, and sex, and the slips were then used to complete the enumerator's schedules. The total number of deaths reported from all sources for the census year will be a little over 800,000, or about 16 per 1,000 of living population, being an improvement in completeness over previous censuses. The results of the attempt to record the number sick on the day of the census are not very satisfactory, and it is feared they will be too incomplete to be used. Taking the schedules for the State of Rhode Island, which are believed to be the most complete, it is found that the number reported sick on the 30th of June was 11.18 per 1,000 of the whole population.

It is usual to estimate two years of sickness to each death, which would make the number constantly sick range from 30 to 40 per 1,000. In the army for five years the proportion was 43 per 1,000.

It seems probable that, while the proportion of sick shown by the Rhode Island count is too low, it is more nearly correct than any other data which we possess.

Mr. BILLINGS continued his remarks upon the Methods of the Tenth Census, and described the methods of compiling the mortality statistics and the forms of tables to be used. The importance of these forms is greater than usual since they will probably serve to a certain extent as models for the State Censuses of 1885. The want of uniformity in tables of mortality was shown by a chart in which the various forms were compared. The various items given in a return of death, viz., sex, age, color, civil condition, nativity, parentage, occupation, month of death, locality and cause of death, were commented on, and it was shown that to present all these facts in their various relations, would require several hundred quarto volumes. A selection, therefore, becomes necessary. The relative value of giving the causes of death in detail is very much less in tables to be prepared from the enumerator's schedules than in those prepared from the returns of a system of registration where the cause of death in each case has been certified to by a physician.

The importance of a proper tabulation by locality is very great, and a certain amount of data should be given by counties. A form of mortality return by counties was shown and explained. The distinction between nativity and race or parentage was explained, and great importance attached to the giving the parentage as fully as possible in the present census.

The modes of compiling by schedule sheets, by cards, and by tallying machines were then explained. The subject of life tables for the United States was briefly discussed—the ground being taken that such a table for the whole country would have little or no practical value, and that life tables by States would be much more desirable and important.

Remarks were made on this paper by Messrs. MASON, ANTISELL, TONER, and HARKNESS.

The communication was followed by one from Mr. S. C. BUSEY, on the

RELATION OF METEOROLOGICAL CONDITIONS TO THE SUMMER DIARRHŒAL DISEASES.

[Abstract. The paper will be found in Vol. 32, Transactions American Medical Association.]

An analysis of the mortality statistics of these diseases leads to the following conclusions:

1. Diarrhœal diseases are far more destructive to infants than to adults.

2. They prevail almost exclusively during the warmest months of the year.

3. They are more prevalent in the region of this country north of the north line of the Gulf States and east of the Rocky Mountains.

The first two conclusions are universally admitted; the third is not so generally recognized.

Two additional propositions are suggested:

1. These diseases occur in groups, when the cases rapidly multiply during successive days for a week or fortnight, followed by an interval during which few or no cases occur.

2. These groups correspond with waves of continuous high temperature during day and night, which spread, at shorter or longer intervals during the summer months, over the northern climatic belt of this country, lasting from three to fourteen days, and varying in intensity at different times and in different years.

The first of these propositions cannot be established, because of the absence of statistical data relating to the beginning of the initial symptoms of the diseases; the second is proven by data supplied by the Signal Service Bureau. A comparison of these data with the mortality statistics shows:

1. That the month of July is the hottest and sickliest month of the year, most conducive to bowel affections, and most fatal to children under five years of age.

2. The epidemics of bowel affections of children, incident to the summer season, have their beginning nearly simultaneously with the first exacerbation of heat, which usually occurs in the latter half of June; and the maximum daily mortalities more frequently cprrespond with the maximum temperatures, which occur in periods of three or more days, at longer or shorter intervals during the snmmer months.

3. With the usual lowering of temperature and absence of ex-

cessive heat periods, which occur after the middle of August, the daily mortality declines.

4. The detrimental influence of summer temperature is intensified by sudden and acute elevations and falls.

5. Children under one year of age are most numerously and seriously affected.

Heat exhibits its deleterious influence in another and very important relation. It is one of the many conditions which, in conjunction, make up a season. A comparison of the statistics of the weekly mortality from diarrhœal diseases in the principal cities of the country grouped according to latitude, will exhibit the gradual increase of these diseases with the gradual advance of the summer solstice northward until it reaches its maximum during the period when all the elements which complete the season of summer are in their fullest activity; also a gradual decline with the return of the winter season.

The total movement of the wind is, perhaps, a more important influence than is generally believed. A comparison of the mortality data with the records of the monthly measurement of the wind, supplied by the Signal Service Bureau for the years 1875, 1876, 1877, 1878, 1879, and 1880, shows:

1. July is the month of greatest mortality and least movement of the wind.

2. The nearer the monthly movements of the wind approach uniformity, the less the mortality for summer diarrhœas.

3. Equality of climate corresponds with uniformity of and moderate or small movements of wind, and small mortalities.

4. Wide ranges of temperature correspond with large movements of wind and high mortalities from diarrhœal diseases.

5. Weekly mortalities from diarrhœal disease increase correspondingly with advance of the summer solstice northward, increasing and greater range of temperature, and larger and more fluctuating movements of wind.

Relative saturation of the air bears no constant relation to mortalities. Moisture in relative excess to the heat of an impure and stagnant atmosphere is the condition which supplies the most satisfactory explanation of its detrimental influence.

Remarks were made upon this paper by Messrs. HARKNESS, BILLINGS, and WOODWARD.

At the conclusion of this discussion the Society adjourned.

201ST MEETING. MAY 28, 1881.

The President in the Chair.

Thirty-four members present.

The minutes of the last meeting were read and adopted.

The first communication was by Mr. D. P. TODD on

THE SOLAR PARALLAX AS DERIVED FROM THE AMERICAN PHOTOGRAPHS OF THE TRANSIT OF VENUS, 1874, DECEMBER 8–9.

In the volume of observations of the transit of Venus recently issued, the photographs are presented in very nearly the form of equations of conditions involving the corrections of the relative right ascension and declination of the sun and Venus, and the correction of the adopted value of the solar parallax. The total number of photographs is 213, of which 84 were obtained at stations in the northern hemisphere, and 129 in the southern.

Every photograph gives one equation of condition in distance, s, of the form

$$0 = a\,\delta A + b\,\delta D + c\,\delta\omega - (\mathrm{O.} - \mathrm{C.})$$

The normal equations in s are—

$$+ 23.99\,\delta A + 24.71\,\delta D - 28.72\,\delta\omega - 82.17 = 0$$
$$+ 24.71\,\delta A + 184.66\,\delta D - 3.16\,\delta\omega - 439.51 = 0$$
$$- 28.72\,\delta A - 3.16\,\delta D + 484.51\,\delta\omega + 21.72 = 0$$

Their solution gives—

$$\delta A = + 1.''181 \pm 0.''202$$
$$\delta D = + 2.''225 \pm 0.''070$$
$$\delta\omega = + 0.''0397 \pm 0.''0418$$

Every photograph gives, likewise, one equation of condition in position-angle, p, of the form

$$0 = a'\,\delta A + b'\,\delta D + c'\,\delta\omega - (\mathrm{O}'. - \mathrm{C}'.)$$

The normal equations in p are—

$$+ 8682117\,\delta A - 1404261\,\delta D - 138999.20\,\delta\omega - 142109.4 = 0$$
$$- 1404261\,\delta A + 1521370\,\delta D - 25093.11\,\delta\omega + 10442.1 = 0$$
$$- 138999.20\,\delta A + 25093.11\,\delta D + 7326.76\,\delta\omega + 2651.6 = 0$$

Their solution gives—

$$\delta A = + 1.''109 \pm 0.''109$$
$$\delta D = + 0.''637 \pm 0.''224$$
$$\delta \omega = + 0.''0252 \pm 0.''0595$$

Combining these values of δ A, δ D, and $\delta\ \omega$ in accordance with their probable errors, we have, finally,

$$\delta A = + 0.''075 \pm 0.''006$$
$$\delta D = + 2.''083 \pm 0.''067$$
$$\delta \omega = + 0.''035 \pm 0.''034$$

The assumed value of ω being 8.″848, we have, therefore, for the mean equatorial horizontal parallax of the sun,

$$8.''883 \pm 0.''034,$$

corresponding to a distance between the centres of the sun and earth, equal to 92,028,000 miles.

(This paper appears in part in *The American Journal of Science* for June, 1881.)

Mr. HARKNESS remarked that the Americans who were engaged in the last transit observations may fairly congratulate themselves upon the results obtained from the photographs, as he had no doubt that they were more satisfactory and consistent than the photographic results obtained by any other nation. There may be said to be two distinct methods of obtaining photographs involving instruments differing widely from the other. The English method employed a telescope of four or five inches aperture producing an image of the sun about three-fourths of an inch in diameter. It is necessary to enlarge this image to a diameter of about four inches, and therefore they used in connection with it a Dallmeyer rapid rectilinear lens, enlarging it by that amount. It is obvious that this enlargement by the use of such a lens must be accompanied by an amount of distortion of the image, which, unless it can be accurately determined and eliminated, must introduce a serious error in the measurements of the negatives, and in the results derived from them. This distortion varies in the direction of radii from the optical center of the image, and is equal in circles about that center. Thus far the amount of this distortion has not been determined. The other method, employed by the Americans, involved the use of a lens with forty feet focal distance giving directly the required size of image, and involving no appreciable distortion inherently due to the construction of the apparatus, and thus avoided the causes of error just described. The focal length required to be determined with great accuracy, and this was readily effected.

Another difficulty arose from the fact that the diameter of the photographic picture on the negative was liable to variation, with a varying length of exposure; and the diameter of the image of Venus is liable to an inverse variation of the same kind. If the distance between the exterior boundaries of the sun and planet were measured, this error would be liable to vitiate the result and, hence, it was necessary to find the centers of the two images, and measure the distances between these central points. Mr. Harkness described the method by which this was satisfactorily accomplished.

There were about twenty plates which gave anomalous results. It was obvious after trial, that the difficulty was with the plates themselves and not due to the observers, since from any one plate a number of observers obtained corresponding results.

Mr. HARKNESS then spoke of the various methods employed to ascertain the sun's parallax: 1st, by measuring the velocity of light, and the time required for light to traverse known chords of the earth's orbit; 2d, by measuring the aberration of light; 3d, by measuring the parallax of the planet Mars; and 4th, by the analysis of the motions of the moon; all of which gave results in very close agreement.

The second communication was by Mr. G. K. GILBERT on

THE ORIGIN OF THE TOPOGRAPHICAL FEATURES OF LAKE SHORES.

This communication was reserved by the author.

After remarks by Mr. ANTISELL, the Society adjourned.

202D MEETING. JUNE 11, 1881.

The President in the Chair.

Fifty-seven members and visitors present.

The minutes of the last meeting were read and adopted.

The Chair announced to the Society that the General Committee had resolved that at the conclusion of the present meeting the Society would stand adjourned until the second Saturday in October.

The first communication of the evening was by Mr. J. J. WOODWARD, the President of the Society, entitled

A BIOGRAPHICAL SKETCH OF THE LATE DR. OTIS.

George Alexander Otis, Surgeon and Brevet Lieutenant-Colonel, United States Army, Curator of the Army Medical Museum, and Editor of the Surgical volumes of the Medical and Surgical History of the War of the Rebellion, died at Washington, D. C., February 23, 1881, at the comparatively early age of fifty years.

Surgeon Otis was descended from a cultivated New England family. His great grandfather, Ephraim Otis, was a physician who practiced at Scituate, Massachusetts. His grandfather, George Alexander Otis, was a well-known citizen of Boston, Massachusetts, whose early years were occupied by commercial pursuits. Mr. Otis was a man of education and literary tastes, who, so soon as his circumstances permitted, retired from business, and devoted himself entirely to books. He is remembered especially on account of his translation of Botta's History of the War of the Independence of the United States of America, published in 1820, an undertaking in which he was encouraged by James Madison and John Quincy Adams, and which he accomplished so well that the book ran through twelve editions. He died at an advanced age in June, 1863.

The father of Surgeon Otis, also George Alexander Otis, was born in 1804. He attended the preparatory course at the Boston Latin School, studied and graduated at Harvard College, after which he devoted himself, with much promise, to the profession of law. Mr. Otis was married February 9, 1830, to Anna Maria Hickman, of Newton, Massachusetts, daughter of Harris Hickman, a lawyer, born at Front Royal, Virginia, who had enjoyed an excellent professional reputation in early life in the Shenandoah Valley, and subsequently at Detroit, in the then Territory of Michigan. Of this marriage the subject of our biographical sketch was the only issue, Mr. Otis dying of consumption, June 18, 1831.

George Alexander Otis was born in Boston, Massachusetts, November 12, 1830. Left an infant to the tender care of his widowed mother, his early years were nurtured by a devoted love, which accompanied him through youth and manhood, smoothed the pillow of his last illness, and followed him to the grave.

When old enough to go to school, George was sent at first to the Boston Latin School, and afterwards to the Fairfax Institute, at Alexandria, Virginia, where he was prepared for college. In 1846 he entered Princeton College as a student of the sophomore class, and graduated with the degree of A. B., in 1849. Princeton conferred upon him the degree of Master of Arts in 1852.

At Princeton, Otis appeared as a slender, rather delicate youth, of highly nervous organization, whose literary tastes were not satisfied with the comparatively narrow curriculum of his Alma Mater. Always standing well in his college classes, that he did not take a still higher place was not due to lack of ability or of studious habits, but rather to his love of general literature, and the large proportion of his time expended in its cultivation. He had already acquired a fondness for French literature, which he never afterwards lost, and a taste for verse so far cultivated that when he came to graduate the Faculty assigned to him the task of preparing the commencement-day poem. Retiring and reserved in his manners, often silent and abstracted, the few who were admitted to his intimacy found his nature gentle and sympathetic, and several of the friendships he then formed lasted throughout his life.

By this time Otis had selected medicine as his profession. After leaving Princeton he went to Richmond, Virginia, where his mother was then residing, and began his studies in the office of Dr. F. H. Deane, of that city. In the fall of 1849 he proceeded to Philadelphia, and matriculated in the Medical Department of the University of Pennsylvania. That institution conferred upon him the degree of Doctor of Medicine in April, 1851. In those days the medical teachings of the University of Pennsylvania were shaped in no small degree by the influence of the Schools of Paris. Indeed, this was then true of almost all American medical teaching, and ambitious American medical students still looked with enthusiasm towards the lecture-rooms and hospitals of the French capital as affording the richest opportunities for the completion of their medical education. Accordingly Otis spent in Paris the first winter after he graduated in Philadelphia. He sailed from New York on the 16th of August, and reached Paris in the latter part of September, 1851.

During his stay in Paris, Otis made diligent use of the opportunities afforded for professional improvement. A manuscript note-book left among his papers shows that he devoted much time to the clinical teachings of the great French masters of that day. He listened to the instructions of Louis, Piorry, Cruveilhier, and Andral. It was at the time his expectation to give especial attention to the subject of ophthalmic surgery, and accordingly he attended with great diligence the clinics and didactic lectures of Desmarres, but he found the attractions of general operative surgery too strong to permit exclusive attention to this chosen branch, and he continually watched the operations, and listened to the lessons of such surgeons as Nélaton, Civiale, Malgaigne, Jobert (de Lamballe), Roux, and Velpeau. Moreover, the popular excitement which preceded the coup d'état of December 2, 1851, and the probability of bloodshed, directed his attention to the subject of military surgery. Already, November 4th, his note-book records a morning spent at the library of l'Ecole de Médecine in the study of Baron Larrey's "Mémoire," with which he was so well pleased that he at once purchased a copy for closer study. After the coup d'état a considerable number of those wounded at the barricades were carried to the hospitals for treatment, and Otis was thus enabled to take his first practical lessons in military surgery from Velpeau, Roux, and Jobert (de Lamballe).

Meanwhile, however, his diligence in medical studies did not prevent him from spending many pleasant hours in the art galleries and museums, where he found much to gratify his æsthetic nature. Moreover, he took a deep interest in the stirring panorama of French politics, as is shown by a series of letters he took time to write to the *Boston Evening Transcript*.

In the spring of 1852 Otis returned to the United States, reaching New York in the latter part of March. Immediately after his return he established himself at Richmond, Virginia, where he opened an office for general medical and surgical practice, and where his tastes and ambition soon led him to embark in his earliest enterprise in the domain of medical literature. In April, 1853, he issued the first number of *The Virginia Medical and Surgical Journal*. Dr. Howell L. Thomas, of Richmond, was associated with him as co-editor, but the financial risk was assumed entirely by Otis. The journal appeared monthly, each number containing over eighty pages octavo, the whole forming two annual volumes, commencing respectively with the numbers of April and October. It was handsomely printed, and contained from time to time a fair share of original articles, chiefly by physicians residing in Richmond and other parts of Virginia; but its most striking characteristic was the number of translations and abstracts from current French medical literature which appeared in its pages. Dr. Thomas, like his colleague, was a good French scholar, and had studied in Paris; both took part in the labor of translation and condensation, and as most of the articles were unsigned, it is not always possible to ascribe particular ones to the proper editor.

Notwithstanding its merits several causes contributed to interfere with the financial success of the journal. On the one hand, it was unsupported by the influence and business connections of an established publishing house, or of the faculty of any medical college. On the other hand, the success it might perhaps otherwise have achieved as a local organ of the medical profession in Virginia was impaired by the existence of an already-established rival, *The Stethoscope*, a monthly medical journal edited by Dr. P. Claiborne Gooch, at that time Secretary of the Medical Society of Virginia.

The field of local patronage was not large enough to support two such journals, and both suffered from the competition. Before the close of 1853, Otis found it necessary to secure an associate who could share in the pecuniary support of his enterprise. Thomas retired from the editorship, and was succeeded after the issue of the December number, by Dr. James B. McCaw, of Richmond, who became also part owner of the journal. *The Stethescope* appears to have suffered still more, for about the same time its editor entered into negotiations with the Virginia Medical Society, as a result of which he sold the journal, and the number of *The Stethescope* for January, 1854, appeared as "the property and organ of the Medical Society of Virginia, edited by a committee of the society."

This arrangement was, undoubtedly, for a time very prejudicial to the prosperity of the *Virginia Medical and Surgical Journal*, but its editors bravely maintained the struggle, and in the heated discussion concerning the purchase of *The Stethoscope*, that took place during the meeting of the Medical Society of Virginia in April, 1854, Otis, with characteristic gallantry, refused to surrender his independence to secure the passage of resolutions complimentary of the managment of his journal.

Otis had, by this time, become dissatisfied with his prospects of professional success in Richmond, and circumstances led him to select Springfield, Massachusetts, as his place of residence. He removed to that town during the summer of 1854. This necessitated changes in the management of the *Virginia Medical and Surgical Journal*. In May, 1854, Dr. J. F. Peebles, of Petersburg, Virginia, became associated with McCaw as one of its editors, while Otis retired from active participation in its direction, retaining, however, literary connection with it as corresponding editor.

Meanwhile, a single year proved sufficient to disgust the Virginia Medical Society with the task of editing a journal. Its management was found fruitful of unfortunate dissensions, and in May, 1855, the society wisely concluded to sell out. Under new auspices *The Stethoscope* continued to appear monthly until the close of the year, when an arrangement was effected by which it was united with *The Virginia Medical and Surgical Journal*, under the title of *Virginia Medical Journal*, with McCaw as editor, and Otis as corresponding editor.

Although his residence in Richmond had failed to secure for Otis a lucrative practice, this could not well have been expected at his early age. It had, however, given him some opportunities for acquiring experience at the bedside as well as in literature, and if he did not secure the profitable favor of the laity, he at least won for himself the respect and confidence of his professional brethren. He was an active member of the Virginia Medical Society, and represented that body in the American Medical Association at the Richmond meeting of May, 1852. He was also a member of the Richmond Medico-Chirurgical Society, which he represented in the American Medical Association at the New York meeting of May, 1853.

Established at Springfield, Massachusetts, Otis occupied himself more exclusively than heretofore with the duties of private practice, and with better pecuniary success than he had enjoyed at Richmond. He continued for a time to contribute translations, abstracts, and various items to the *Virginia Medical Journal*; but as the demands of his business became more urgent these became fewer, although he continued to be nominally corresponding editor of that journal until the close of 1859. As time wore on, he began to obtain considerable local reputation as a skillful surgeon, and would probably have acquired both wealth and distinction in civil surgical practice but for the outbreak of the War of the Rebellion. This changed the whole tenor of his life. So soon as it became clear to his mind that the struggle was likely to be a prolonged one, he resolved to devote himself to the service of his country. He received from Governor Andrew the appointment of Surgeon to the 27th Regiment of Massachusetts Volunteers, of which Horace C. Lee was Colonel, and was mustered into the service of the United States, September 14, 1861.

The 27th Regiment was raised in the western part of the State of Massachusetts, and was mustered into the service of the United States at Springfield. It left the State November 2, 1861, and proceeded by rail to the vicinity of Annapolis, Maryland, where it went into camp. Here it remained until January 6, 1862, when it was embarked on transports, and accompanied the North Carolina Expedition under General Burnside. It took part in the affair on Roanoke Island, February 8th; landed near Newburn, North Carolina, March 13th, and met with considerable losses during the battle of Newburn on the following day. The regiment remained in North Carolina until October 16, 1863, when it embarked for Fortress Monroe, Virginia, and after a short encampment at Newport News, proceeded to Norfolk, Virginia, where it remained through the following winter.

During almost the whole of this time Surgeon Otis accompanied his regiment and shared its fortunes; sometimes, indeed, performing other duties in addition to his regimental ones, as during the summer and fall of 1862, when he acted as Medical Purveyor to the Department of North Carolina. The exceptional periods were a few days in September, 1862, when he went as medical officer in charge of the steamer "Star of the South" with sick from Newburn to New York, and a few months in the early part of 1863, when he served on detached duty in the Department of the South. While in the Department of the South he attracted the attention of Surgeon Charles H. Crane, U. S. Army, then Medical Director of the Department (afterwards Assistant Surgeon-General of the Army), on whose recommendation he was placed, March 28th, by command of General Hunter, in charge of the hospital steamer "Cosmopolitan," then at Hilton Head, South Carolina, and directed the operations of that vessel in the transportation of the sick and wounded within the limits of the department until May 10, when he was ordered to carry a number of sick and wounded to New York harbor, and after landing them, to turn over the vessel to Surgeon Wm. Ingalls, of the 5th Massachusetts regiment. This order was promptly executed, the vessel was turned over as directed, May 13th, and Otis received a leave of absence for twenty days, at the expiration of which he returned to his regiment.

January 22, 1864, he was again detached and ordered to Yorktown, Virginia, to assume the duties of surgeon-in-chief of General Wistar's command. This responsible position he filled in a satisfactory manner from the first of February, when he reported for duty at Yorktown, until April 11, when he was relieved and assigned as surgeon-in-chief to General Heckman's division of the 18th Army Corps, then encamped near Portsmouth, Virginia. May 10th he received a sick leave for fifteen days, which, as his health was not restored at its expiration, was extended for thirty days more. June 26, 1864, he tendered his resignation as surgeon of the 27th Massachusetts regiment, and received an appointment as Assistant Surgeon of United States Volunteers, to date from June 30, 1864.

At this time business connected with his resignation and re-appointment brought Otis to Washington, where he renewed his acquaintance with Surgeon Crane, then on duty in the Surgeon General's Office. Surgeon Crane, while Medical Director of the Department of the South, had been most favorably impressed with the culture and ability of the Massachusetts surgeon, and now so effectually commended him to the Acting Surgeon General as to induce that officer to ask his detail for duty in his office. An order to that effect was issued by the Secretary of War July 22, 1864, and Otis was immediately assigned as an assistant to Surgeon John H. Brinton, U. S. Volunteers, at that time Curator of the Army Medical Museum, and engaged in the duty of collecting materials for the Surgical History of the War of the Rebellion. August 30, 1864, Otis was promoted to the rank of Surgeon of Volunteers, and October 3, 1864, was ordered to relieve Surgeon Brinton of his various duties.

From the first, Otis devoted himself with signal zeal and ability to the large and important duties of his new position. Immediately after he took charge of the Surgical Division he inaugurated a system of record books, which proved ultimately of great service in securing the accurate and complete record of individual cases for use in the Surgical History. The rapidly increasing surgical collection of the Army Medical Museum also received great attention from him, and he expended much time in its supervision and study.

Immediately after the close of the war, the Surgeon General of the Army became desirous of securing, by appropriate legislation, the funds necessary to complete and publish the Medical and Surgical History of the War. Accordingly he called upon Otis, and his colleague, Woodward, who had charge of the collection of materials for the Medical History and of the medical branches of the

Museum, to make reports on the extent and nature of the materials collected for the purpose in question. These reports were published by the Surgeon General November 1, 1865, as "Circular No. 6," for the year 1865. This circular was widely distributed, attracted great attention at the time, and satisfactorily attained the object which led to its publication. It formed a quarto volume of 166 pages, with a number of illustrations intended to indicate the character of those regarded as desirable for the Medical and Surgical History. The first half of the volume was occupied by the Surgical Report prepared by Otis. It was a thoughtfully prepared document, which excited the universal admiration of military surgeons in Europe as well as in America.

It became necessary after the close of the war to retain many of the staff surgeons of volunteers in the service for duty in the general hospitals or other purposes after the great armies had been disbanded, and Otis was, of course, retained with that rank as long as possible; but it was foreseen that the great work he had commenced would occupy a number of years, and he was induced to make arrangements for entering the army as an assistant surgeon. Accordingly he passed the examination prescribed by law, and February 28, 1866, received an appointment as Assistant Surgeon, U. S. Army, but he was not finally mustered out of service as surgeon of volunteers until June 4, 1866, and hence did not accept his commision as Assistant Surgeon U. S. A., until the 6th of that month.

Meanwhile Otis was devoting himself to the study and arrangement of the materials collected for the Surgical History with indefatigable energy, and while engaged upon that work received authority to publish two preliminary studies on special subjects connected therewith, which greatly increased the reputation he had won by his report in Circular No. 6. The first was *A Report on Amputation at the Hip-joint in Military Surgery*, published as Circular No. 7, Surgeon General's Office, July 1, 1867. In this he not merely presented and analyzed the histories of the several amputations at this joint reported to the Surgeon General's Office during the civil war, but discussed with the critical abilities of a master the whole literature of the subject so far as it was at the time accessible to him. An examination of this monograph shows that he had already pretty well begun to emancipate himself from the leading-strings of the French school, and had fully acquired the desire, so manifest in his subsequent work, to compare and weigh all accessible human knowledge on each branch of his subject before arriving at his own conclusions.

These characteristics were, if possible, still more fully displayed in the second of the studies referred to: *A Report on Excisions of the Head of the Femur for Gunshot Injury,* published as Circular No. 2, Surgeon General's Office, January 2, 1869; a monograph in which the subject was treated in a manner similar to that of Circular No. 7, but with a still greater wealth of literary resources. The appearance of each of these monographs was welcomed with acclamations of praise, in which the authoritative expressions of approval by the recognized masters of European surgery were united with the encomiums of the American military surgeons.

Great interest in the forthcoming Surgical History of the War was excited by these publications, and very high expectations were formed, which, however, were fully realized by the character of the *First Surgical Volume.* This volume was issued in 1870. It treated of the special wounds and injuries of the head, face, neck, spine, aud chest, was richly illustrated, and discussed the vast amount of material collected during the civil war, in connection with the several subjects treated, with characteristic learning and ability. The *Second Surgical Volume* was issued in 1876. It treated of the wounds and injuries of the abdomen, pelvis, back, and upper extremities. Fully equal in interest and execution to the first volume, it was much more voluminous. The two volumes represent a prodigious amount of patient labor on the part of the editor. The extremely favorable manner in which they were received in surgical circles at home and abroad is well known.

During the interval between the appearance of these two volumes, and subsequently, Otis found time to prepare and publish several valuable reports on subjects connected with military surgery, of which the most important were: *A Report of Surgical Cases treated in the Army of the United States from* 1865 *to* 1871, issued as Circular No. 3 from the Surgeon General's Office, August 17, 1871, *A Report on a Plan for Transporting Wounded Soldiers by Railway in time of War,* Surgeon General's Office, 1875; and *A Report on the Transport of Sick and Wounded by Pack Animals,* issued as Circular No. 9 from the Surgeon General's Office in 1877. A full list of his official and other publications would occupy too much space to be presented in this place.

In the midst of this successful but laborious career, during the month of May, 1877, his health, never very robust, gave way, and, although he survived for several years, he was a constant invalid, to whom death came in the end as a welcome release from suffering. He was engaged at the time of his death on the third surgical volume, which he has left in an unfinished condition; a colossal fragment that must require great labor to complete in a manner worthy of the first two volumes.

Otis received the appointments of captain, major, and lieutenant-colonel by brevet, to date from September 29, 1866, "for faithful and meritorious services during the war." He was promoted to be surgeon in the army, with the rank of major, March 17, 1880. He was elected a foreign member of the Medical Society of Norway, October 26, 1870; a foreign corresponding member of the Surgical Society of Paris, August 11, 1875; and an honorary life member of the Massachusetts Medical Society in February, 1877. He was also at the time of his death a member of the Philosophical Society of Washington, and of the Academy of Natural Sciences of Philadelphia.

In expressing his high appreciation of the character and value of the surgical works of his late colleague, the writer of these pages does but echo the universal language of competent critics throughout the civilized world. On all sides the opinion has been expressed that they have not only made the name of Otis illustrious, but have reflected the greatest credit upon the intelligent liberality of the Government of the United States, and upon the Medical Corps of the Army.

During his connection with the Museum, Otis always took deep interest in the anatomical collection, now embracing about two thousand human crania. As early as January, 1873, the Surgeon General at his instance made a fruitless endeavor to procure an appropriation for the publication of an illustrated catalogue of this valuable collection. To facilitate this object Otis prepared a check-list of the specimens, which was printed in 1876, but the pecuniary means for preparing and publishing the larger work have not yet been provided.

Until his last illness Otis retained much of the fondness for polite literature which characterized him in early life. He had, moreover, considerable taste for music and the fine arts. These qualities made his companionship charming to those who enjoyed his intimacy.

Hesitating, often embarassed, in his manner in ordinary conversation, especially with strangers, he became eloquent when warmed by the discussion of any topic in which he took interest, and he took interest in a great variety of subjects besides those directly connected with the work of his life.

Many warm personal friends share the grief of his family at his untimely death, which, as has been well said by the Surgeon-General, "will be deeply deplored not only by the Medical Corps of the Army, but by the whole medical profession at home and abroad."

LIST OF THE PUBLICATIONS OF G. A. OTIS, M. D., ETC.

Case of Pericarditis in a child of four years and seven months of age. [Reported to the Medico-Chirurgical Society of Richmond, March 1, 1853.] The Virginia Medical and Surgical Journal, Vol. I, 1853, p. 33.

On Hemorrhage from the Umbilicus in new-born Infants. Same Journal, Vol. II, 1853, p. 49.

A Report of a Case in which an Enlargement of the Isthmus of the Thyroid Body was successfully extirpated. Same Vol., p. 115.

On the Per-chloride of Iron in the Treatment of Aneurisms. [Remarks appended to a translation of an article by *Malgaigne*: "De l'emploi du perchlorure de fer dans le Traitement des Anéurismes." L'Abeille Médicale, Octobre, 1853, p. 292 *et seq.*] Same Vol., pp. 295 and 497.

On the Local Treatment of Erysipelas. [Abstract of remarks made in the Medico-Chirurgical Society of Richmond, January 17, 1854.] Same Journal, Vol. III, 1854, p. 13.

Translation, with Notes, of Velpeau's Review of the Surgical Clinique of La Charité, during the Scholastic Year of 1853–4. [Translated from Le Moniteur des Hopitaux, 1854, p. 801 *et seq.*] Same Journal, Vol. IV, 1855, pp. 31, 111, and 321, and Vol. V, 1855, pp. 213, 298, and 378.

Remarks and Excerpts relating to Variola and Vaccinia. Virginia Medical Journal, Vol. VII, 1856, p. 109.

On Strangulated Hernia in Children. Same Journal, Vol. X, 1858, p. 201.

Letter to the Surgeon General of Massachusetts on the Sanitary Condition of the 27th Mass. Vols., from Camp Reed, near Springfield, Mass., October 5, 1861. The Boston Medical and Surgical Journal, Vol. 65, 1862, p. 204.

Letter to the same, on the same, from Camp Springfied, near Annapolis, Md. Same Vol., p. 435.

Letter to the same, from Newbern, N. C., March 28, 1862, [giving an account of the participation of the regiment in the battle of Newbern, and of his management of the wounded.] Same Journal, Vol. 66, 1862, p. 237.

The Surgical portion of (pp. 1-88) *Circular No. 6, War Department, Surgeon General's Office, November 1, 1865.* Reports on the extent and nature of the materials available for the preparation of a Medical and Surgical History of the Rebellion. Printed for the Surgeon General's Office by J. B. Lippincott & Co., Philadelphia, 1865, 4to., pp. 88.

Circular No. 7, War Department, Surgeon General's Office, Washington, July 1, 1867. A Report on Amputations at the Hip-joint in Military Surgery. 4to., pp. 87.

Observations on some Recent Contributions to the Statistics of Excisions and Amputations at the Hip for Injury. The American Journal of the Medical Sciences, Vol. LVI, July, 1868, p. 128.

Rejoinder to a Reply to a Review of Dr. Eve's Contribution on the History of Hip-joint Operation. The Buffalo Medical and Surgical Journal, Vol. VIII, August, 1868, p. 21.

Circular No. 2, War Department, Surgeon General's Office, Washington, January 2, 1869. A Report on Excision of the Head of the Femur for Gunshot Injury. 4to., pp, 141.

Medical and Surgical History of the War of the Rebellion, 1861-1865, Part 1, Vol. II, being the First Surgical Volume. Washington, Government Printing Office, 1870, 4to., pp. 650. Second issue, 1875.

Circular No. 3, War Department, Surgeon General's Office, Washington, August 17, 1871. A Report of Surgical Cases treated in the Army of the United States from 1865 to 1871. 4to., pp. 196.

Memorandum of a Case of Re-amputation at the Hip, with Remarks on the Operation. The American Journal of the Medical Sciences, Vol. LXI, January, 1871, p. 141.

A Report on the Plan for Transporting Wounded Soldiers by Railway in time of War. Washington, Surgeon General's Office, 1875, 8vo., pp. 56.

Description of Selected Specimens from the Surgical Section of the Army Medical Museum at Washington. [International Exhibition of 1876.] Gibson Bros., Washington, 1876, 8vo., pp. 22.

Description of the U. S. Army Medicine Transport Cart, Model of 1876, prepared in conjunction with Brevet Lieutenant Colonel D. L. Huntington, Assistant Surgeon U. S. A. [International Exhibition of 1876.] Gibson Bros., Washington, 1876, 8vo., pp. 16.

Check-List of Preparations and Objects in the Section of Human Anatomy of the U. S. Army Medical Museum. [International Exhibition of 1876.] Gibson Bros., Washington, 1876, pp. 135. Second edition, Gibson Bros., Washington, 1880, 8vo., pp. 194.

Medical and Surgical History of the War of the Rebellion, 1861–1865, Part II, being the Second Surgical Volume. Washington, Government Printing Office, 1876, 4to., pp. 1024. Second issue, 1877.

Circular No. 9, War Department, Surgeon General's Office, March 1, 1877. A Report to the Surgeon General on the Transport of Sick and Wounded by Pack Animals. 4to., pp. 32.

Report of a Board of Officers to decide on a Pattern of Ambulance Wagon for Army Use. [Prepared by him as recorder of the board.] Washington, Government Printing Office, 1878, 8vo., pp. 79.

Contributions from the Army Medical Museum. Boston Medical and Surgical Journal, Vol. XCVI, March, 1877, p. 361.

Article *Surgery* in Johnson's New Universal Cyclopædia. New York, A. J. Johnson & Son, 1878, Vol. IV, pp. 1678–1686.

Notes on Contributions to the Army Medical Museum by Civil Practitioners. Boston Medical and Surgical Journal, Vol. XCVIII, February, 1878, p. 163.

Recent Progress in Military Surgery. Same Vol., April, p. 531.

Photographs of Surgical Cases and Specimens, taken at the Army Medical Museum, with Histories of three hundred and seventy-five cases. Washington, Surgeon General's Office, 1866. 1881, 8 vols., 4to.

The next communication was by Mr. Alexander Graham Bell

UPON A MODIFICATION OF WHEATSTONE'S MICROPHONE AND ITS APPLICABILITY TO RADIOPHONIC RESEARCHES.

In August, 1880, I directed attention to the fact that thin disks or diaphragms of various materials become sonorous when exposed to the action of an intermittent beam of sunlight, and I stated my belief that the sounds were due to molecular disturbances produced in the substance composing the diaphragm.* Shortly afterwards Lord Raleigh undertook a mathematical investigation of the subject, and came to the conclusion that the audible effects were caused by the bending of the plates under unequal heating.† This explanation has recently been called in question by Mr. Preece,‡ who has

* Amr. Ass. for Advancement of Science, Aug. 27, 1881.
† Nature, Vol. XXIII, p. 274.
‡ Roy. Soc., Mar. 10, 1881.

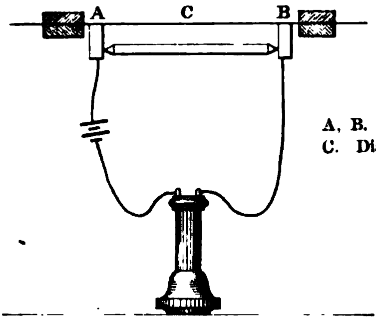
expressed the opinion that although vibrations may be produced in the disks by the action of the intermittent beam, such vibrations are not the cause of the sonorous effects observed. According to him the ærial disturbances that produce the sound arise spontaneously in the air itself by sudden expansion due to heat communicated from the diaphragm; every increase of heat giving rise to a fresh pulse of air. Mr. Preece was led to discard the theoretical explanation of Lord Raleigh on account of the failure of experiments undertaken to test the theory.

He was thus forced, by the supposed insufficiency of the explanation, to seek in some other direction the cause of the phenomenon observed, and, as a consequence, he adopted the ingenious hypothesis alluded to above. But the experiments which had proved unsuccessful in the hands of Mr. Preece were perfectly successful when repeated in America under better conditions of experiment, and the supposed necessity for another hypothesis at once vanished. I have shown in a recent paper read before the National Academy of Science,* that audible sounds result from the expansion and contraction of the material exposed to the beam, and that a real to and fro vibration of the diaphragm occurs capable of producing sonorous effects. It has occurred to me that Mr. Preece's failure to detect with a delicate microphone the sonorous vibrations that were so easily observed in our experiments, might be explained upon the supposition that he had employed the ordinary form of Hughes' microphone shown in Fig. 1, and that the vibrating area was confined to the central portion of the disk. Under such circumstances it might easily happen that both the portions (A B) of the microphone might touch portions of the diaphragm which were practically at rest. It would, of course, be interesting to ascertain whether any such localization of the vibration as that supposed really occured, and I have great pleasure in showing to you to-night the apparatus by means of which this point has been investigated. [See Fig. 2.]

The instrument is a modification of the form of microphone devised in 1827 by the late Sir Charles Wheatstone, and it consists essentially of a stiff wire, (A,) one end of which is rigidly attached to the centre of a metallic diaphragm (B.) In Wheatstone's original arrangement, the diaphragm was placed directly against the ear

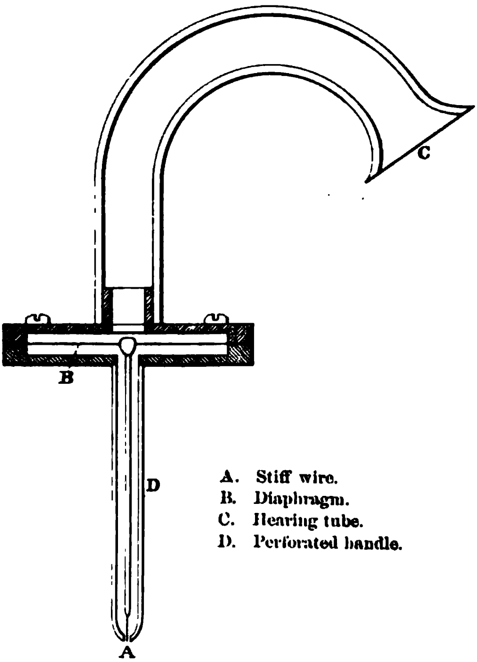
* April 21, 1881.

Fig. 1.



A, B. Carbon supports.
C. Diaphragm.

Fig. 2.



A. Stiff wire.
B. Diaphragm.
C. Hearing tube.
D. Perforated handle.

and the free extremity of the wire was rested against some sounding body, like a watch. In the present arrangement the diaphragm is clamped at the circumference like a telephone-diaphragm, and the sounds are conveyed to the ear through a rubber hearing-tube (C.) The wire passes through the perforated handle (D,) and is exposed only at the extremity. When the point (A) was rested against the centre of a diaphragm, upon which was focussed an intermittent beam of sunlight, a clear musical tone was perceived by applying the ear to the hearing-tube (C.) The surface of the diaphragm was then explored with the point of the microphone, and sounds were obtained in all parts of the illuminated area, and in the corresponding area on the other side of the diaphragm. Outside of this area on both sides of the diaphragm the sounds became weaker and weaker until at a certain distance from the centre they could no longer be perceived.

At the points where one would naturally place the supports of a Hughes' microphone [see Fig. 1,] no sound was observed. We were also unable to detect any audible effects when the point of the microphone was rested against the support to which the diaphragm was attached. The negative results obtained in Europe by Mr. Preece may, therefore, be reconciled with the positive results obtained in America by Mr. Tainter and myself. A still more curious demonstration of localization of vibration occurred in the case of a large metallic mass. An intermittent beam of sunlight was focussed upon a brass weight (1 kilogram,) and the surface of the weight was then explored with the microphone shown in Fig. 2. A feeble but distinct sound was heard upon touching the surface within the illuminated area, and for a short distance outside, but not in other parts.

In this experiment, as in the case of the thin diaphragm, absolute contact between the point of the microphone, and the surface explored was necessary in order to obtain audible effects. Now, I do not mean to deny that sound waves may be originated in the manner suggested by Mr. Preece, but I think that our experiments have demonstrated that the kind of action described by Lord Raleigh actually occurs and that it is sufficient to account for the audible effects observed.

The next communication was by Mr. J. M. TONER on

EARTH VIBRATIONS AT NIAGARA FALLS.

In June, 1874, the speaker, in company with Dr. J. D. Jackson, of Kentucky, visited the Clifton House on the Canada side of Niagara. On the night of his arrival he was kept awake by the illness of his companion, and his attention was drawn to the frequent rattling of the doors and windows of his room. He was first led to suppose, while speculating upon the cause, that the vibration might be due to pulsations in the air produced by the falling water; but upon further reflection concluded that it could not be satisfactorily explained in that way, as it continued independently of the direction of the wind. On the following day he made it the subject of conversation with others, but no one seemed to agree with him. He had occasion, however, to note when his chair was tilted back against the stone wall of the house that a tremulous motion, or grating was perceptible. At the time this tremor was a novelty to him, but subsequently he had met with allusions to it by several writers. He was led to the following explanation, viz: that the fall of such a large body of water through so great a vertical distance, must necessarily impart vibrations to the massive rocks which form the trough of the river above and below the falls, and that these vibrations are transmitted through the earth itself. To test this theory, he made on the next day the following experiments: A large carving dish holding water was placed on the rock between the falls and the hotel. Upon the water was poured some sweet oil, and it was seen that wave-rings appeared on the surface of the water. These rings were made more distinct by placing a mirror so as to view them by reflection. No rhythm was detected in these vibrations. The dish was placed in many localities, more than thirty in number, and at varying distances from the falls. Waves were observed in it from the Burning Spring above the falls, and as far as half a mile below the small suspension bridge. They were also noted on the steps of the little Episcopal Church, a mile west of the Hotel on the Canada side. Similar results were obtained on the American side.

At the conclusion of Mr. Toner's remarks the Society adjourned to October 8th.

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BULLETIN

OF THE

PHILOSOPHICAL SOCIETY

OF

WASHINGTON.

VOL. V.

Containing the Minutes of the Society from the 203d Meeting, October 8, 1881, to the 226th Meeting, Dec. 16, 1882.

PUBLISHED BY THE CO-OPERATION OF THE SMITHSONIAN INSTITUTION.

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1883. May 5,
Gift of
The Society

JUDD & DETWEILER, PRINTERS,
WASHINGTON, D. C.

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CONSTITUTION, STANDING RULES,

AND

LIST OF OFFICERS AND MEMBERS

OF

THE PHILOSOPHICAL SOCIETY

OF

WASHINGTON.

CONSTITUTION

OF

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

ARTICLE I. The name of this Society shall be THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

ARTICLE II. The officers of the Society shall be a President, four Vice-Presidents, a Treasurer, and two Secretaries.

ARTICLE III. There shall be a General Committee, consisting of the officers of the Society and nine other members.

ARTICLE IV. The officers of the Society and the other members of the General Committee shall be elected annually by ballot; they shall hold office until their successors are elected, and shall have power to fill vacancies.

ARTICLE V. It shall be the duty of the General Committee to make rules for the government of the Society, and to transact all its business.

ARTICLE VI. This constitution shall not be amended except by a three-fourths vote of those present at an annual meeting for the election of officers, and after notice of the proposed change shall have been given in writing at a stated meeting of the Society at least four weeks previously.

STANDING RULES

FOR THE GOVERNMENT OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

JANUARY, 1881.

---

1. The Stated Meetings of the Society shall be held at 8 o'clock P. M. on every alternate Saturday; the place of meeting to be designated by the General Committee.

2. Notice of the time and place of meeting shall be sent to each member by one of the Secretaries.

When necessary, Special Meetings may be called by the President.

3. The Annual Meeting for the election of officers shall be the last stated meeting in the month of December.

The order of proceedings (which shall be announced by the Chair) shall be as follows:

First, the reading of the minutes of the last Annual Meeting.

Second, the presentation of the annual reports of the Secretaries, including the announcement of the names of members elected since the last annual meeting.

Third, the presentation of the annual report of the Treasurer.

Fourth, the announcement of the names of members who having complied with Section 12 of the Standing Rules, are entitled to vote on the election of officers.

Fifth, the election of President.

Sixth, the election of four Vice-Presidents.

Seventh, the election of Treasurer.

Eighth, the election of two Secretaries.

Ninth, the election of nine members of the General Committee.

Tenth, the consideration of Amendments to the Constitution of

the Society, if any such shall have been proposed in accordance with Article VI of the Constitution.

Eleventh, the reading of the rough minutes of the meeting.

4. Elections of officers are to be held as follows:

In each case nominations shall be made by means of an informal ballot, the result of which shall be announced by the Secretary; after which the first formal ballot shall be taken.

In the ballot for Vice-Presidents, Secretaries, and Members of the General Committee, each voter shall write on one ballot as many names as there are officers to be elected, viz., four on the first ballot for Vice-Presidents, two on the first for Secretaries, and nine on the first for Members of the General Committee; and on each subsequent ballot as many names as there are persons yet to be elected; and those persons who receive a majority of the votes cast shall be declared elected.

If in any case the informal ballot result in giving a majority for any one, it may be declared formal by a majority vote.

5. The Stated Meetings, with the exception of the annual meeting, shall be devoted to the consideration and discussion of scientific subjects.

The Stated Meeting next preceding the Annual Meeting shall be set apart for the delivery of the President's Annual Address.

6. Sections representing special branches of science may be formed by the General Committee upon the written recommendation of twenty members of the Society.

7. Persons interested in science, who are not residents of the District of Columbia, may be present at any meeting of the Society, except the annual meeting, upon invitation of a member.

8. Similar invitations to residents of the District of Columbia, not members of the Society, must be submitted through one of the Secretaries to the General Committee for approval.

9. Invitations to attend during three months the meetings of the Society and participate in the discussion of papers, may, by a vote of nine members of the General Committee, be issued to persons nominated by two members.

10. Communications intended for publication under the auspices of the Society shall be submitted in writing to the General Committee for approval.

11. New members may be proposed in writing by three members of the Society for election by the General Committee: but no person shall be admitted to the privileges of membership unless he signifies his acceptance thereof in writing within two months after notification of his election.

12. Each member shall pay annually to the Treasurer the sum of five dollars, and no member whose dues are unpaid shall vote at the annual meeting for the election of officers, or be entitled to a copy of the Bulletin.

In the absence of the Treasurer, the Secretary is authorized to receive the dues of members.

The names of those two years in arrears shall be dropped from the list of members.

Notice of resignation of membership shall be given in writing to the General Committee through the President or one of the Secretaries.

13. The fiscal year shall terminate with the Annual Meeting.

14. Members who are absent from the District of Columbia for more than twelve months may be excused from payment of the annual assessments, in which case their names shall be dropped from the list of members. They can, however, resume their membership by giving notice to the President of their wish to do so.

15. Any member not in arrears may, by the payment of one hundred dollars at any one time, become a life member, and be relieved from all further annual dues and other assessments.

All moneys received in payment of life membership shall be invested as portions of a permanent fund, which shall be directed solely to the furtherance of such special scientific work as may be ordered by the General Committee.

STANDING RULES

OF THE

GENERAL COMMITTEE OF THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

JANUARY, 1881.

---

1. The President, Vice-Presidents, and Secretaries of the Society shall hold like offices in the General Committee.

2. The President shall have power to call special meetings of the Committee, and to appoint Sub-Committees.

3. The Sub-Committees shall prepare business for the General Committee, and perform such other duties as may be entrusted to them.

4. There shall be two Standing Sub-Committees; one on Communications for the Stated Meetings of the Society, and another on Publications.

5. The General Committee shall meet at half-past seven o'clock on the evening of each Stated Meeting, and by adjournment at other times.

6. For all purposes except for the amendment of the Standing Rules of the Committee or of the Society, and the election of members, six members of the Committee shall constitute a quorum.

7. The names of proposed new members recommended in conformity with Section 11 of the Standing Rules of the Society, may be presented at any meeting of the General Committee, but shall lie over for at least four weeks before final action, and the concur-

rence of twelve members of the Committee shall be necessary to election.

The Secretary of the General Committee shall keep a chronological register of the elections and acceptances of members.

8. These Standing Rules, and those for the government of the Society, shall be modified only with the consent of a majority of the members of the General Committee.

RULES

FOR THE

PUBLICATION OF THE BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

JANUARY, 1881.

---

1. The President's annual address shall be published in full.

2. The annual reports of the Secretaries and of the Treasurer shall be published in full.

3. When directed by the General Committee, any communication may be published in full.

4. Abstracts of papers and remarks on the same will be published, when presented to the Secretary by the author in writing within two weeks of the evening of their delivery, and approved by the Committee on Publications. Brief abstracts prepared by one of the Secretaries and approved by the Committee on Publications may also be published.

5. Communications which have been published elsewhere, so as to be generally accessible, will appear in the Bulletin by title only, but with a reference to the place of publication, if made known in season to the Committee on Publications.

NOTE. *The attention of members to the above rules is specially requested.*

OFFICERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

ELECTED DECEMBER 17, 1881.

President .... WILLIAM B. TAYLOR.

Vice Presidents ...... J. K. BARNES, J. E. HILGARD,
J. C. WELLING, J. J. WOODWARD.

Treasurer .... CLEVELAND ABBE.

Secretaries .... MARCUS BAKER, T. N. GILL.

MEMBERS AT LARGE OF THE GENERAL COMMITTEE.

J. S. BILLINGS, WILLIAM HARKNESS,
C. E. DUTTON, GARRICK MALLERY,
J. R. EASTMAN, SIMON NEWCOMB,
E. B. ELLIOTT, J. W. POWELL,
C. A. SCHOTT.

STANDING COMMITTEES.

On Communications:

MARCUS BAKER, *Chairman.* C. E. DUTTON, T. N. GILL.

On Publications:

T. N. GILL, *Chairman.* CLEVELAND ABBE, S. F. BAIRD,* MARCUS BAKER.

* As Secretary of the Smithsonian Institution.

LIST OF MEMBERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

Corrected to May, 1882.

(*a*) indicates a *founder* of the Society.

(*b*) indicates *deceased*.

(*c*) indicates *absent* from the District of Columbia and excused from payment of dues until announcing their return.

(*d*) indicates *resigned*.

(*e*) indicates *dropped* for non-payment or nothing known of him.

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Abbe, Cleveland	Army Signal Office. 2017 I St. N. W.	1871, Oct. 29
Abert, Sylvanus Thayer	Engineer's Office, War Department. 1724 Penn. Ave. N.W.	1875, Jan. 30
Adams, Henry	1605 H St	1881, Feb. —
Aldis, Asa Owen	1617 Rhode Island Ave. N. W.	1873, Mar. 1
Allen, James	Army Signal Office. 1707 G St. N. W.	1882, Feb. 25
Alvord, Benjamin	1207 Q St. N. W	1872, Mar. 23
Antisell, Thomas (*a*)	Patent Office. 1311 Q St. N. W	1871, Mar. 13
Avery, Robert Stanton	Coast and Geodetic Survey Office. 320 A St. S. E.	1879, Oct. 11
Babcock, Orville Elias	2024 G St. N. W	1871, June 9
Bailey, Theodorus (*b*)		1873, Mar. 1
Baird, Spencer Fullerton (*a*)	Smithsonian Institution. 1445 Mass. Ave. N. W.	1871, Mar. 13
Baker, Frank	326 C St. N. W	1881, May 14
Baker, Marcus	Coast and Geodetic Survey Office. 1205 Rhode Island Ave. N. W.	1876, Mar. 11
Bancroft, George	1623 H St. N. W	1875, Jan. 16
Barnes, Joseph K (*a*)	Surg. Genl's Office. 1723 H St. N. W.	1871, Mar. 13
Bartley, Thomas Welles	Office, 1343 F St. N.W. Res., 1016 13th St. N. W.	1873, Mar. 29
Bates, Henry Hobart	Patent Office. 1313 R St. N. W	1871, Nov. 4
Beardslee, Lester Anthony (*c*)	Navy Department.	1875, Feb. 27
Bell, Alexander Graham	1221 Conn. Ave. N.W. Res., 1302 Conn. Ave. N. W.	1879, Mar. 29
Bell, Chichester Alexander	1221 Conn. Ave. N.W. Res., 2023 Mass. Ave. N. W.	1881, Oct. 8
Benét, Stephen Vincent (*a*)	Ordnance Office, War Department. 1717 I St. N. W.	1871, Mar. 13
Bessels, Emil	Smithsonian Institution. 1441 Mass. Ave. N. W.	1875, Jan. 16
Billings, John Shaw (*a*)	Surg. Genl's Office. 3027 N St. N. W.	1871, Mar. 13
Birney, William	330 4½ St. N.W. Res., 1901 Harewood Ave., Le Droit Park.	1879, Mar. 29
Birnie, Rogers (*c*)	Cold Spring, Putnam Co., N. Y.	1876, Mar. 11
Burchard, Horatio Chapin	Director of the Mint, Treasury Dept. Res., Riggs House.	1879, May 10
Burnett, Swan Moses	1215 I St. N. W	1879, Mar. 29
Busey, Samuel Clagett	1525 I St. N. W	1874, Jan. 17
Capron, Horace (*a*)	The Portland	1871, Mar. 13
Case, Augustus Ludlow (*c*)	Navy Department. Bristol, R. I.	1872, Nov. 16
Casey, Thomas Lincoln (*a*)	Engineer Bureau, War Department. 1410 K St. N. W.	1871, Mar. 13

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Casiarc, Louis Vasmer	Army Signal Office. 1446 N St. N. W.	1882, Feb. 25
Chase, Salmon Portland (*a b*)		1871, Mar. 13
Chickering, John White, Jr.	Deaf Mute College, Kendall Green	1874, Apr. 11
Christie, Alexander Smyth	Coast and Geodetic Survey Office. 1102 14th St. N. W.	1880, Dec. 4
Clapp, William Henry	Army Signal Office. 806 18th St. N.W.	1882, Feb. 25
Clark, Edward	Architect's Office, Capitol. 417 4th St. N. W.	1877, Feb. 24
Clark, Ezra Westcott	Revenue Marine Bureau, Treasury Department. Res., Woodley road.	1882, Mar. 25
Clarke, Frank Wigglesworth (*c*)	University of Cincinnati. Albion Place, Cincinnati, Ohio.	1874, Apr. 11
Coffin, John Huntington Crane (*a*)	1901 I St. N. W	1871, Mar. 13
Collins, Frederick (*b*)		1879, Oct. 21
Comstock, John Henry	Cornell University, Ithaca, N. Y.	1880, Feb. 14
Coues, Elliott	Smithsonian Inst. 1321 N St. N. W	1874, Jan. 17
Craig, Benjamin Faneuil (*a b*)		1871, Mar. 13
Craig, Robert	Army Signal Office. 1008 I St. N. W.	1873, Jan. 4
Craig, Thomas	Johns Hopkins Univ., Baltimore, Md.	1879, Nov. 22
Crane, Charles Henry (*a*)	Surg. Genl's Office. 1909 F St. N. W.	1871, Mar. 13
Curtis, Josiah	428 7th street N. W. Riggs House	1874, Mar. 28
Cutts, Richard Dominicus	Coast and Geodetic Survey Office. 1725 H St. N. W.	1871, Apr. 29
Dall, William Healey (*a*)	P. O. Box 406. 1119 12th St. N. W	1871, Mar. 13
Davis, Charles Henry (*b*)		1874, Jan. 17
Davis, Charles Henry	Navy Department. 1705 Rhode Island Ave. N. W.	1880, June 19
Dean, Richard Crain (*b*)		1872, Apr. 23
De Caindry, William Augustin	Commissary General's Office. 924 19th St. N. W.	1881, Apr. 30
De Land, Theodore Louis	Treasury Dept. 126 7th St. N. E.	1880, Dec. 18
Dewey, George (*d*)	Light House Board. 826 14th St. N.W.	1879, Feb. 15
Doolittle, Myrick Hascall	Coast and Geodetic Survey Office. 1925 I St. N. W.	1876, Feb. 12
Dorr, Fredric William (*b*)		1874, Jan. 17
Dunwoody, Henry Harrison Chase	Army Signal Office. 1412 G St. N. W.	1873, Dec. 20
Dutton, Clarence Edward	Geological Survey	1872, Jan. 27
Dyer, Alexander B. (*a b*)		1871, Mar. 13
Eastman, John Robie	Naval Observatory. 2721 N St. N. W.	1871, May 27
Eaton, Amos Beebe (*a b*)		1871, Mar. 13
Eaton, John	Bureau of Education, Interior Dept. 712 East Capitol St.	1874, May 8
Eldredge, Stewart (*c*)		1871, June 9
Elliot, George Henry (*a d*)	Engineer Bureau, War Department	1871, Mar. 13
Elliott, Ezekiel Brown (*a*)	Mint Bureau, Treasury Department. 607 I St. N. W.	1871, Mar. 13
Endlich, Frederic Miller	Smithsonian Institution	1873, Mar. 1
Ewing, Charles (*c*)		1874, Jan. 17
Ewing, Hugh (*c*)	Lancaster, Ohio	1874, Jan. 17
Farquhar, Edward Jessop	Patent Office Library. 1915 H St. N.W.	1876, Feb. 12
Farquhar, Henry	Coast and Geodetic Survey Office. 726 20th St. N. W.	1881, May 14
Ferrel, William	Coast and Geodetic Survey Office. 471 C St. N. W.	1872, Nov. 16
Fletcher, Robert	Surgeon Genl's Office. 314 Ind. Ave.	1873, Apr. 10
Flint, Albert Stowell	Naval Observatory. 1209 Rhode Island Ave. N. W.	1882, Mar. 25
Flint, James Milton	Smithsonian Inst. Riggs House	1881, Mar. 26
Foote, Elisha (*a c*)		1871, Mar. 13
Foster, John Gray (*b*)		1873, Jan. 18
French, Henry Flagg	Treasury Department. 137 East Capitol St.	1882, Mar. 25
Frisby, Edgar	Naval Observatory. 3006 P St. N. W.	1872, Nov. 16
Fristoe, Edward T	Columbian College. College Hill N.W.	1873, Mar. 29
Gale, Leonard Dunnell	1230 Mass. Ave. N. W	1874, Jan. 17
Gallaudet, Edward Miner	Deaf Mute College, Kendall Green	1875, Feb. 27
Gannett, Henry	Geological Survey. 1881 Harewood Ave., Le Droit Park.	1874, Apr. 11
Gardner, James Terry (*c*)	State Library, Albany, N. Y.	1874, Jan. 17
Garnett, Alexander Young P. (*d*)	1317 N. Y. Ave. N. W	1878, Mar. 16
Gihon, Albert Leary	Navy Department. 1736 I St. N. W.	1880, Dec. 18

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Gilbert, Grove Karl	Geological Survey. Le Droit Park	1873, June 7
Gill, Theodore Nicholas (*a*)	Smithsonian Inst. 321-323 4½ St. N.W.	1871, Mar. 13
Godding, William Whiting	Government Asylum for the Insane	1879, Mar. 29
Goode, George Brown	National Museum. 1620 Mass. Av. N.W.	1874, Jan. 31
Goodfellow, Edward	Coast and Geodetic Survey Office	1875, Dec. 18
Goodfellow, Henry (*d*)	Bureau of Military Justice, War Dept.	1871, Nov. 4
Gore, James Howard	Columbian College. 1305 Q St. N. W.	1880, Mar. 14
Graves, Edward Oziel (*c*)		1874, Apr. 11
Graves, Walter Hayden (*c*)	Denver, Colorado	1878, May 25
Greely, Adolphus Washington (*c*)		1880, June 19
Green, Bernard Richardson	1738 N St. N. W	1879, Feb. 15
Green, Francis Mathews	Bureau of Navigation, Navy Dept	1875, Nov. 9
Greene, Benjamin Franklin (*a c*)	West Lebanon, N. H	1871, Mar. 13
Greene, Francis Vinton	War Department. 1915 G St. N. W	1875, Apr. 10
Gunnell, Francis Mackall (*c*)	600 20th St. N. W	1879, Feb. 1
Hains, Peter Conover (*c*)	Office Light House Engineer, Charleston, S. C.	1879, Feb. 15
Hall, Asaph (*a*)	Naval Observatory. 2715 N St. N. W.	1871, Mar. 13
Hanscom, Isaiah (*b*)		1873, Dec. 20
Harkness, William (*a*)	Naval Observatory. 1415 G St. N. W.	1871, Mar. 13
Hassler, Ferdinand Augustus (*c*)	Tustin City, Los Angeles Co., Cal	1880, May 8
Hayden, Ferdinand Vandeveer (*ac*)	Geological Survey. 1803 Arch street, Philadelphia, Penna.	1871, Mar. 13
Hazen, Henry Allen	Army Signal Office. 1209 R. I. Av. N.W.	1882, Mar. 25
Hazen, William Babcock	Army Signal Office. 1601 K St. N. W.	1881, Feb. —
Henry, Joseph (*a b*)		1871, Mar. 13
Henshaw, Henry Wetherbee	Bureau of Ethnology. 903 M St. N.W.	1874, Apr. 11
Hilgard, Julius Erasmus (*a*)	Coast and Geodetic Survey Office. 1709 Rhode Island Ave. N. W.	1871, Mar. 13
Hill, George William	Nautical Almanac Office. 318 Ind. Ave. N. W.	1879, Feb. 1
Holden, Edward Singleton (*c*)	Madison, Wisconsin	1873, June 21
Holmes, William Henry.	Geological Survey	1879, Mar. 29
Hough, Franklin Benjamin (*c*)	Agricultural Department	1879, Mar. 29
Howell, Edwin Eugene (*c*)	Rochester, N. Y	1874, Jan. 31
Howgate, Henry W		1873, Jan. 18
Humphreys, Andrew Atkinson (*a*)	S. E. Corner 15th and K Sts. N. W.	1871, Mar. 13
Huntington, David Lowe	Army Med. Museum. 1709 M St. N.W.	1877, Dec. 21
Jackson, Henry Arundel Lambe (*c*)	War Department	1875, Jan. 30
James, Owen (*c*)	Hyde Park, Penna	1880, Jan. 3
Jeffers, William Nicolson (*d*)	Navy Department	1877, Feb. 24
Jenkins, Thornton Alexander (*a*)	2115 Penn. Ave. N. W	1871, Mar. 13
Johnson, Arnold Burgess	Light House Board, Treasury Dept. 501 Maple Ave., Le Droit Park.	1878, Jan. 19
Johnson, Joseph Taber	937 New York Ave. N. W	1879, Mar. 29
Johnston, William Waring	1401 H St. N. W	1873, Jan. 21
Kampf, Ferdinand (*b*)		1875, Dec. 18
Keith, Reuel (*c*)		1871, Oct. 20
Kidder, Jerome Henry	Navy Department. 1601 O St. N. W.	1880, May 8
Kilbourne, Charles Evans.	Army Signal Office. Lexington House.	1880, June 19
King, Albert Freeman Africanus	726 13th St. N. W	1875, Jan. 16
King, Clarence (*d*)		1879, May 10
Knox, John Jay	Treasury Dept. 1127 10th St. N. W	1874, May 8
Kummell, Charles Hugo	Coast and Geodetic Survey Office. 608 Q St. N. W.	1882, Mar. 25
Lane, Jonathan Homer (*a b*)		1871, Mar. 13
Lawver, Winfield Peter	Mint Bureau, Treasury Department. 1912 I St. N. W.	1881, Feb. 19
Lee, William	2111 Penn. Ave. N. W	1874, Jan. 17
Lincoln, Nathan Smith	1514 H St. N. W	1871, May, 27
Lockwood, Henry H. (*d*)		1871, Oct. 29
Loomis, Eben Jenks	Nautical Almanac Office. 1413 College Hill Terrace N. W.	1880, Feb. 14
Lull, Edward Phelps	Navy Department. 1313 M St. N. W	1875, Dec. 4
Lyford, Stephen Carr (*d*)	Ordnance Office, War Department	1873, Jan. 18
Macauley, Henry Clay (*c*)		1880, Jan. 3
McGuire, Frederick Bauders	1306 F St. N. W. Res., 614 E St. N. W.	1879, Feb. 15
Mack, Oscar A. (*b*)		1872, Jan. 27
McMurtrie, William	Agricultural Dept. 1728 I St. N. W	1876, Feb. 26

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Mallery, Garrick	Bureau of Ethnology. P. O. Box 585. Res., 1323 N St. N. W.	1875, Jan. 30
Marvin, Joseph Badger (c)		1878, May 25
Marvine, Archibald Robertson (b)		1874, Jan. 31
Mason, Otis Tufton	Columbian College. 1305 Q St. N. W.	1875, Jan. 30
Meek, Fielding Bradford (a b)		1871, Mar. 13
Meigs, Montgomery (c)	War Department. Rock Island, Ill.	1877, Mar. 24
Meigs, Montgomery Cunningham (a)	1239 Vermont Ave. N. W.	1871, Mar. 13
Menocal, Aniceto Garcia	Navy Yard, Washington, D. C	1877, Feb. 24
Mew, William Manuel	Army Medical Museum. 942 New York Ave. N. W.	1873, Dec. 20
Milner, James William (b)		1874, Jan. 31
Morris, Martin Ferdinand (c)	717 12th St. N. W.	1877, Feb. 24
Mussey, Reuben Delavan	P. O. Box 618. Res., 508 5th St. N. W.	1881, Dec. 3
Myer, Albert J. (a b)		1871, Mar. 13
Myers, William (c)	Office of Commissary General, War Department.	1871, June 23
Newcomb, Simon (a)	Navy Department. 1336 11th St. N.W.	1871, Mar. 13
Nichols, Charles Henry (c)		1872, May 4
Nicholson, Walter Lamb (a)	Topographer of Post Office Dept. 1322 I St. N. W.	1871, Mar. 13
Nordhoff, Charles	New York Herald Bureau. 1027 New York Ave. N. W.	1879, May 10
Osborne, John Walter	212 Delaware Ave. N. E	1878, Dec. 7
Otis, George Alexander (a b)		1871, Mar. 13
Packard, Robert Lawrence (c)	Patent Office. 2022 G St. N. W	1875, Feb. 27
Parke, John Grubb (a)	Engineer Bureau, War Department. 16 16½ St. N. W.	1871, Mar. 13
Parker, Peter (a)	2 La Fayette Square	1871, Mar. 13
Parry, Charles Christopher (c)	Burlington, Iowa	1871, May 13
Patterson, Carlile Pollock (b)		1871, Nov. 17
Paul, Henry Martyn (c)	University of Tokio, Japan	1877, May 19
Peale, Albert Charles (c)	Schuylkill Haven, Schuylkill Co., Pa.	1874, Apr. 11
Peale, Titian Ramsay (a c)		1871, Mar. 13
Peirce, Benjamin (a b)		1871, Mar. 13
Peirce, Charles Sanders (c)	Coast and Geodetic Survey Office. Res., Baltimore, Md.	1873, Mar. 1
Pilling, James Constantine	Geological Survey. 903 M St. N. W	1881, Feb. 19
Poe, Orlando Metcalfe	Headquarters of the Army. 1507 Rhode Island Ave. N. W.	1873, Oct. 4
Porter, David Dixon (d)	1710 H St. N. W	1874, Apr. 11
Powell, John Wesley	Geological Survey. 910 M St. N. W	1874, Jan. 17
Prentiss, Daniel Webster	1224 9th St. N. W	1880, Jan. 3
Pritchett, Henry Smith (c)	Washington University, St. Louis, Mo.	1879, Mar. 29
Rathbone, Henry Reed (c)		1874, Jan. 17
Ridgway, Robert (c)	Smithsonian Inst. 1214 Va. Av. N.W.	1874, Jan. 31
Riley, Charles Valentine	Agricultural Dept. 1700 13th St. N.W.	1878, Nov. 9
Riley, John Campbell (b)		1877, May 19
Ritter, William Francis McKnight	Nautical Almanac Office. 16 Grant Place.	1879, Oct. 21
Rodgers, Christopher Raymond Perry (c)	1723 I St. N. W	1872, Mar. 9
Rodgers, John (b)		1872, Nov. 16
Rogers, Joseph Addison (c)	Naval Observatory	1872, Mar. 9
Russell, Israel Cook	Geological Survey	1882, Mar. 25
Sands, Benjamin Franklin (a)	816 15th St. N. W	1871, Mar. 13
Saville, James Hamilton	342 D St. (La. Ave.) N. W. Res., 1315 M St. N. W.	1871, Apr. 29
Sawyer, Frederic Adolphus (c)		1873, Oct. 4
Schaeffer, George Christian (a b)		1871, Mar. 13
Schott, Charles Anthony (a)	Coast and Geodetic Survey Office. 212 1st St. S. E.	1871, Mar. 13
Searle, Henry Robinson	1223 10th St. N. W	1877, Dec. 21
Seymour, George Dudley	607 7th St. N.W. Res., 1007 9th St. N.W.	1881, Dec. 3
Shellabarger, Samuel	Room 23, Corcoran Building. Res., 812 17th St. N. W.	1875, Apr. 10
Sherman, John	1317 K St. N. W	1874, Jan. 17
Sherman, William Tecumseh (a d)	War Department. 817 15th St. N. W	1871, Mar. 13
Shufeldt, Robert Wilson	Surg. Genl's Office. 819 17th St. N.W.	1881, Nov. 5

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Sicard, Montgomery (*c*)	Ordnance Bureau, Navy Department. 1404 L St. N. W.	1877, Feb. 24
Sigsbee, Charles Dwight	Hydrographic Office, Navy Dept. 3319 U St., West Washington.	1879, Mar. 1
Skinner, Aaron Nicholas (*e*)	Naval Observatory. 1726 10th St. N.W.	1875, Feb. 27
Smith, David (*c*)	Navy Department	1876, Dec. 2
Smith, Edwin	Coast and Geodetic Survey	1880, Oct. 23
Spofford, Ainsworth Rand	Library of Congress. 1621 Mass. Ave. N. W.	1872, Jan. 27
Stearns, John (*c*)		1874, Mar. 28
Stone, Ormond (*c*)	Leander McCormick Observatory, University of Virginia.	1874, Mar. 28
Story, John Patten	Army Signal Office. 921 17th St. N.W.	1880, June 19
Taylor, Frederick William	Smithsonian Institution. 1120 Vermont Ave. N. W.	1881, Feb. 19
Taylor, George (*e*)	804 E St. N. W. Res., 1120 Vermont Ave. N. W.	1873, Mar. 1
Taylor, William Bower (*a*)	Smithsonian Inst. 457 C St. N. W	1871, Mar. 13
Thompson, Almon Harris (*c*)	Ivanpah, Greenwood Co., Kansas	1875, Apr. 10
Tilden, William Calvin (*c*)	Army Medical Museum	1871, Apr. 29
Todd, David Peck (*c*)	Amherst, Mass	1878, Nov. 23
Toner, Joseph Meredith	615 Louisiana Ave	1873, June 7
Twining, William J. (*b*)		1878, Nov. 23
Upton, Jacob Kendrick (*d*)	Cooke & Co., cor. 15th St. and Penn. Ave. 1721 De Sales St.	1878, Feb. 2
Upton, William Wirt	2d Comptroller's Office, Treasury Dept. 810 12th St. N. W.	1882, Mar. 25
Upton, Winslow	Army Signal Office. 1441 Chapin St. N. W.	1880, Dec. 4
Vasey, George	Agricultural Dept. 1437 S St. N. W.	1875, June 5
Waldo, Frank	Army Signal Office. 1427 Chapin St. N. W.	1881, Dec. 3
Walker, Francis Amasa (*c*)	Mass. Inst. of Technology, Boston, Mass.	1872, Jan. 27
Ward, Lester Frank	Geological Survey. 1464 R. I. Av. N.W.	1876, Nov. 18
Warren, Charles (*e*)	Bureau of Education. 1208 N St. N. W.	1874, May 8
Webster, Albert Lowry	Geological Survey. P. O. Box 591	1882, Mar. 25
Welling, James Clarke	Columbian College	1872 Nov. 16
Wheeler, George M. (*c*)	Engineer Bureau, War Department	1873, June 7
Wheeler, Junius B (*a c*)	West Point, New York	1871, Mar. 13
White, Charles Abiathar	Geological Survey. Le Droit Park	1876, Dec. 16
White, Zebulon Lewis (*c*)	Providence, Rhode Island	1880, June 19
Wilson, Allen D.	Geological Survey	1874, Apr. 11
Wilson, James Ormond	Franklin School Building. 1439 Mass. Ave. N. W.	1873, Mar. 1
Winlock, William Crawford	Naval Observatory. 1903 F St. N. W.	1880, Dec. 4
Wolcott, Christopher Columbus (*d*)	War Department	1875, Feb. 27
Wood, Joseph (*c*)	Asst. Engineer B. & P. R. R.	1875, Jan. 16
Wood, William Maxwell (*c*)	Navy Department	1871, Dec. 2
Woodward, Joseph Janvier (*a*)	Army Med. Museum. 620 F St. N. W.	1871, Mar. 13
Woodworth, John Maynard (*b*)		1874, Jan. 31
Yarnall, Mordecai (*b*)		1871, Apr. 29
Yarrow, Harry Crécy	814 17th St. N. W	1874, Jan. 31
Zumbrock, Anton	Coast and Geodetic Survey Office. 306 C St. N. W.	1875, Jan. 30

Number of *founders*	44	
" members *deceased*		28
" " *absent*		52
" " *resigned*		12
" " *dropped*		5
" " *active*		149
Total number enrolled		246

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

203D MEETING. OCTOBER 8, 1881.

The Society, in accordance with the notice of adjournment at its last June meeting, resumed its sessions.

The President (Mr. J. J. WOODWARD) in the Chair.

Thirty-eight members present.

Mr. G. K. GILBERT read a communication on

THE QUATERNARY CLIMATE OF THE GREAT BASIN.

The matters contained in this communication were a summary of certain chapters which will appear from the pen of Mr. Gilbert in the Second Annual Report of the Director of the United States Geological Survey now in press. The observations of which the communication was a resume were made in his capacity of Geologist in charge of the Exploration of the Utah Division.

Remarks were made on Mr. Gilbert's communication by Mr. THOMAS ANTISELL.

Mr. E. B. ELLIOTT also made a communication on

ACCRUED INTEREST ON GOVERNMENT SECUTITIES.

Mr. W. B. TAYLOR exhibited to the Society a photographic print from a single negative including about 140 degrees of panorama. The ordinary camera does not usually comprise more than about 60 degrees, and requires as a necessary condition of good definition perfect stability of the lens and the plate. In the present case, an inspection of the two houses presented in the rural view, (especially of the longer one near the middle of the picture,) with the curved road winding between them to the right, shows that a revolving camera was employed; the long sensitive plate having evidently been simultaneously moved transversely in the reverse direction to that of the objective. This perfect co-ordination of the revolving and sliding movements could be obtained by a mechanical gearing; and the extended landscape be thus successively impressed upon advancing portions of the plate—probably through a vertical slit in a diaphragm immediately in front of the plate. That the corelation of movement has been very perfect is evidenced by the admirable precision of every detail in the photograph. It will be observed that the three men standing in different parts of the field of view are one and the same individual, who has had time to pass behind the instrument, and to twice take a new position in advance of the moving camera. By bending the long card into a concave arc somewhat more than the third of a cylinder, and placing the eye at the axis of curvature, it will be seen that the various slight distortions of perspective (particularly in the houses) are completely corrected.

Mr. J. M. Toner exhibited, *apropos* to the approaching centennial of the surrender of Cornwallis at Yorktown, certain well preserved specimens of coins and medals of national historic interest, viz:

(1.) Bronze copy of medal given to Washington on the evacuation of Boston.

(2.) A bronze copy of a medal of Lafayette.

(3.) A bronze copy of a medal of Columbus.

(4.) A very fine half dollar of 1785.

(5.) A very fine Washington cent of 1791.

204th Meeting. October 22, 1881.

The President in the Chair.

Forty members present.

Mr. A. B. Johnson presented the following communication on

RECENT INVESTIGATIONS BY THE LIGHT-HOUSE BOARD ON THE ANOMALIES OF SOUND FROM FOG SIGNALS.

Among our erroneous popular notions is one which occasionally brings practical men, even ship-masters, to grief. It is the idea that sound is always heard in all directions from its source according to its intensity or force, and according to the distance of the hearer from it. Instances of this fallacy have accumulated, and they are emphasized by shipwrecks caused by the insistance of mariners on the infallibility of their ears, who have accepted unquestioned the guidance of sound signals during fog as they have that of light-houses during clear weather. The fact is, audition is subject to aberrations, and under circumstances where little expected. We have learned by sad experience that implicit reliance on sound signals may, as it has, lead to danger if not to death.

The wreck of the steamer Rhode Island, on Bonnet Point in Narragansett Bay, which happened on November 6, 1880, when a million dollars in property was lost, was caused, it was said, by the failure of the fog-signal on Beaver Tail Point to sound at that time. Thereupon the Light-House Board, which has charge of the sixty and more fog-signals on our coasts, made an investigation which showed that the fog-signal was in full operation when the wreck took place; but it also brought out the fact, that while there was no lack in the volume of the sound emitted by the signal, there was often a decided lack in the audition of that sound, so much so that it would not be heard at the intensity expected, nor at the place expected; indeed it would be heard faintly where it ought to be heard loudly, and loudly where it ought to be heard faintly; that it could not be heard at all at some points, and then further away it could be heard better than near by; that it could be heard and lost and heard and lost again, all within reasonable ear shot, and all this while the signal was in full blast and sounding continuously.

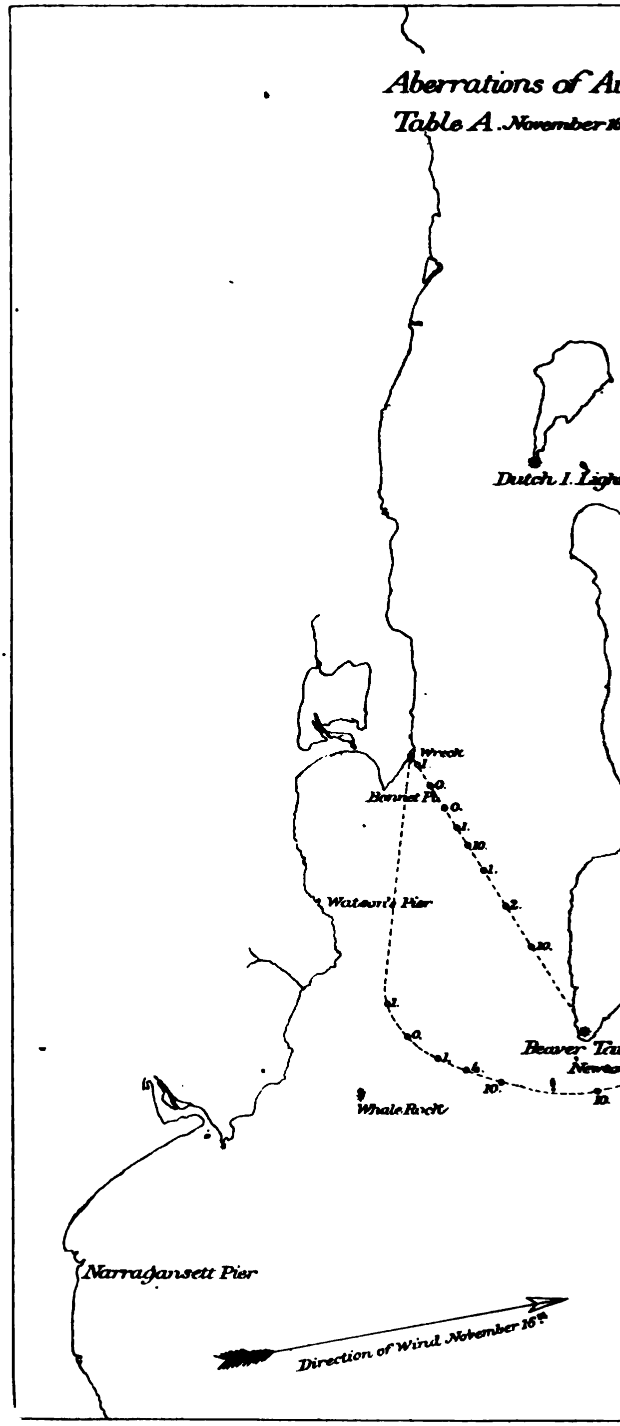
The following table, A, will give the results obtained by the officer of the navy who investigated these phenomena, and reported to the Light-House Board:

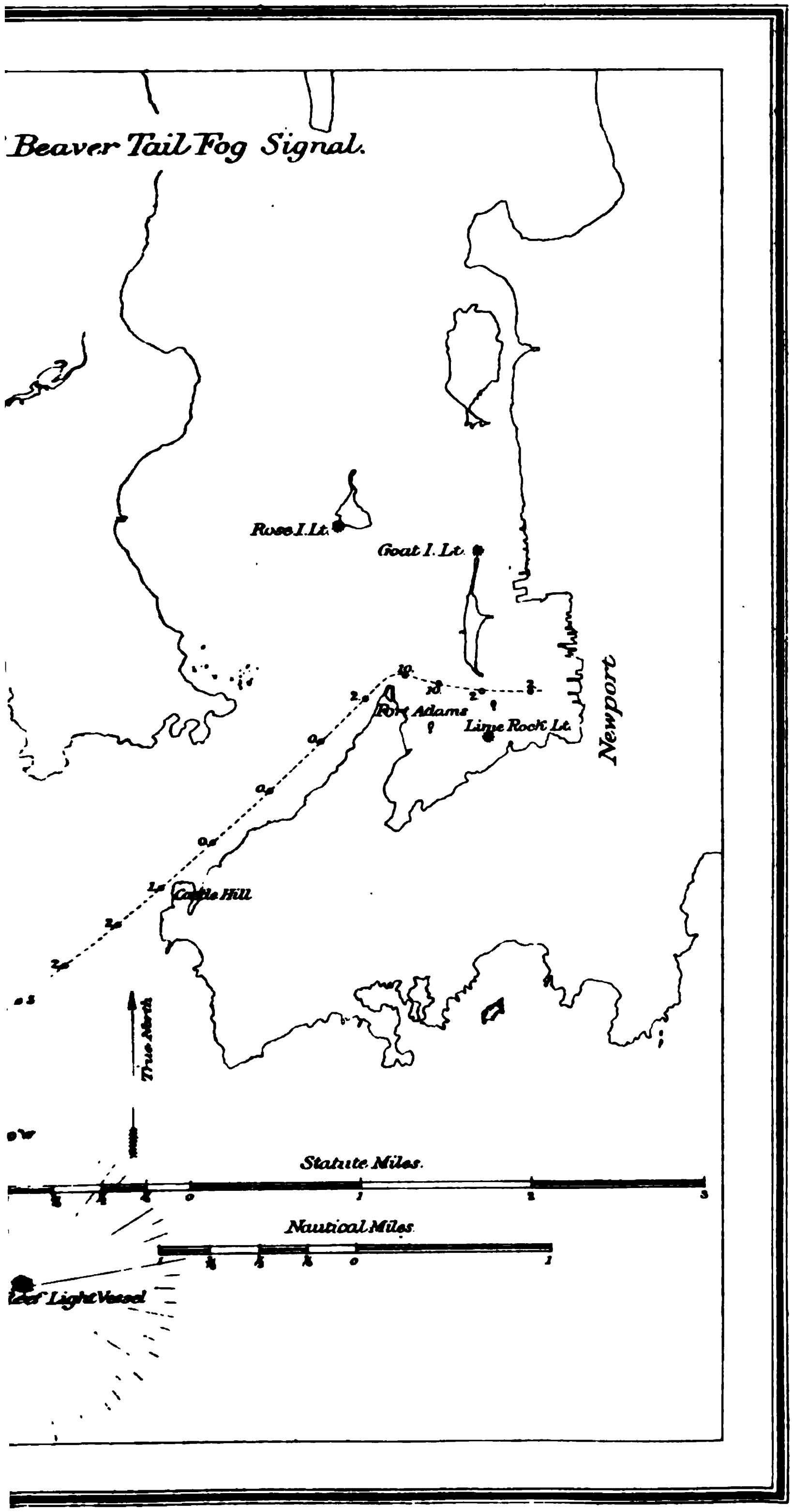
TABLE A.

Observations on Beaver Tail Fog-Signal, Rhode Island, made on November 16, 1880, from a sail-boat, Thermometer at beginning 58°, ending 67°; Wind moderate from the West; Weather clear and cold, with a bright sun. Time, beginning 11.15 A. M.

Number of Observation.	Distance from Beaver Tail Fog-Signal in statute miles.	Intensity of sound in scale of 10.	REMARKS.
1	½	10	
2	⅞	2	
3	1 1/16	1	
4	1¼	10	
5	1⅜	1	
6	1½	0	
7	1⅝	0	
8	1⅞	1	Close to Bonnet Point changed course and ran almost due south.
9	1⅛	1	1½ miles from last station.
10	1	0	¼ mile from last station.
11	⅞	1	" " "
12	⅝	4	" " "
13	½	10	" " "
14	⅜	10	About opposite Beaver Tail, ½ mile from last station, and in the axis of trumpet.
15	½	10	About ½ mile from last station, and running for Newport, heading nearly northeast.
16	1	10	About ½ mile from last station.
17	1¼	5	" ½ " "
18	1½	2	" ¼ " "
19	1⅞	2	" ¼ " "
20	2⅛	1	" ½ " "
21	2½	0	" ¼ " "
22	3½	0	" ½ " "
23	3⅞	2	" ½ " "
24	4	10	About ¼ mile from last station, just off Ft. Adams.
25	4¼	10	Under the lee of Fort Adams.
26	4½	2	
27	4⅝	2	
28	4¾	2	
29	5	2	Newport.

Last summer, I had an opportunity while on a light-house steamer, to experience something of the variations in the audition of the Beaver Tail fog-signal. When the steamer left the light-


Aberrations of Au
Table A. November 16
Dutch I. Ligh
Wreck
Bonnet Pt.
Watson's Pier
Beaver Ta
Whale Rock
Narragansett Pier
Direction of Wind November 16th



Beaver Tail Fog Signal.
Rose I. Lt.
Goat I. Lt.
Fort Adams
Lime Rock Lt.
Newport
Castle Hill
True North
Statute Miles.
Nautical Miles.
Light Vessel

house landing, the fog-signal was to sound for a given time, and to commence when the steamer had reached a given point, half a mile distant. When that point was reached, we could see by the steam-puffs coming from the 'scape pipe, that the signal was being blown; but we could not hear its sound; nor did we, as we continued on our course, running away from the light station for the next five minutes. When near to Whale Rock, less than a mile and a half distant from the signal, the steamer was stopped, silence was ordered fore and aft, and we all listened intently. The expert naval officers thought they heard a trace of the fog-signal, but my untrained ears failed to differentiate it from the moan of the whistling buoy close to us. Yet the blasts of the ten-inch steam whistle, for which we were listening, can often be heard at a distance of ten miles.

Soon after, I had another opportunity to further observe the operations of this signal. We left Narragansett Pier, R. I., on Aug. 6, 1881, at 4 P. M., in a dense fog, with a strong breeze from the W. S. W., and a heavy chop sea. We wished to ascertain how far the Beaver Tail fog-signal could be heard dead to windward and in the heaviest of fogs. At Whale Rock, one and one-third miles from it, we did not hear a trace of it. Then the steamer was headed directly for Beaver Tail Point, and we ran slowly for it by compass, until the pilot stopped the steamer, declaring we were almost aboard of the signal itself. Every one strained his ears to hear the signal but without success; and we had begun to doubt of our position when, the fog lifting slightly, we saw the breakers in altogether too close proximity for comfort. We passed the point as closely as was safe; and, when abreast of it and at right angles with the direction of the wind, the sound of the fog-signal broke on us suddenly and with its full power. We then ran down the wind to Newport, and carried the sound with us all the way. The fog continuing during the next day, the signal kept up its sound, and we heard it distinctly and continuously at our wharf, though five miles distant.

On the night of May 12, 1881, about midnight, the Galatea, a propeller of over 1500 tons burden, with a full load of passengers and freight, bound through Long Island Sound from Providence to New York, grounded in a dead calm and a dense fog on Little Gull Island, about one-eighth of a mile from and behind the fog-signal, and got off two days later without damage to herself or loss of life or freight. It was as usual alleged that the fog-signal, a steam siren, at Little Gull Light, was not in operation at the time of the accident, and the Light-House Board, also, as usual, immediately ordered an investigation. This was made by the Assistant Inspector of the Light-House District, a naval officer, who reported that after taking the sworn evidence of the light-keepers at Little Gull and the other light-stations within hearing distance, of other Government officers who were, for the time being, so located that they might have had knowledge of the facts, and of the officers of vessels that were within ear shot, including those of the Galatea, he reached the conclusion that the fog-signal was sounding at the time of the accident; and that, although the fog-signal was heard at Mystic, fifteen miles distant in another direction, and although it was heard on a steam tug a mile beyond the Galatea; that it was heard faintly, if at all, on that vessel; and if heard at all, was so heard as to be misleading, though the Galatea was but one-eighth of a mile from the source of the sound.

This report is in itself full of interest. It appears that this officer spent several days steaming around Little Gull, while the fog-signal was in full blast, in various kinds of weather, and that he found the aberrations in audition here were as numerous and even more eccentric than those before mentioned as experienced at Beaver Tail. The results of his observations are given in Tables B and C; and in each case the condition of the atmosphere as to humidity, pressure, temperature and motion are shown, as is also the then tidal condition.

Table B.

Fog Signal Tests at Little Gull Island, Long Island Sound, July 11, 1881. Time 10 A. M. Wind, N.N.E., force 2. Barometer, 29.77; Thermometer, 61. Weather at commencement, dark, overcast with squalls of Scotch mist from N.N.E. It began to clear at 11:30 A.M.

Number of Observation.	Time of Observation.	Distance from Little Gull Island fog signal in stat. miles.	Intensity of sound in scale of ten.	Remarks.
	h. m.			
1	10 10	1⅝	1	
2	10 15	2⅜	½	A faint murmur is put at ½ of 1, in scale of 10.
3	10 18	2½	0	
4		3⅜	0	

Number of Observation.	Time of Observation.	Distance from Little Gull Island fog-signal in stat. miles.	Intensity of sound in scale of ten.	Remarks.
	h. m.			
5	10 25	3⅜	0	
6		3½	0	
7		3½	½	About ½ mile from last station.
8	10 50	3½	1	
9		3⅝	0	
10		3⅝	1	About ½ mile from last station.
11		3¾	2	About ½ mile from last station.
12	11 09	3½	2	Changed course and ran a little S. of W.
13		3¾	3	
14	11 15	2⅞	3	
15	11 25	2½	4	
16		2¾	5	
17	11 35	2½	7	
18		2⅛	7	
19		1½	8	
20	11 55	½	9	
21		½	10	
22	12 03	⅜	10	About ½ mile from last station.
23	12 07	⅜	7	
24		1⅞	2	
25	12 14	1⅞	1	
26	12 19	2¼	1½	
27	12 23	2¾	½	Changed course.
28	12 40	2¾	½	Faint murmur.
29	12 52	3½	0	Changed course.
30	1 01	2	½	
31	1 06	1⅝	1-2	
32	1 12	1⅝	5	
33	1 18	¾	10	
34		⅝	10	Almost west of fog-signal.
35		1¼	10	
36	1 35	1½	8	Changed course.
37		1⅝	8	
38	1 42	⅞	10	Stood N. E.; sound gradually increasing.
39	1 52	½	3	
40	1 55	⅞	2	Changed course.
41		¾	2	
42	2 01	⅜	2	
43	2 02	⅛	10	
44		⅜	10	
45		¾	8	
46		1	7	
47		1¾	5	
48	4 29	2	2	
49		2¾	1	
50	4 38	3⅜	0	Lost the sound.
51		3¼	0	
52	4 45	4¼	0	Bartletts Reef light-ship; wheels stopped and no sound.

Table C.

Observations at Little Gull Island, Long Island Sound, July 15, 1881, commencing at 6.30 A. M. Thermometer, 59° Fahr. Barometer, 29.80. Wind, W.N.W., force 3, hauling to the westward and increasing gradually.

Number of Observation.	Time of Observation.	Distance from Little Gull Island fog-signal in stat. miles.	Intensity of sound in a scale of ten.	Remarks.
	h. m.			
1	6 32	1¾	10	
2	6 57	2¼	10	Changed course, running S. by W. ½ W.
3		2¼	8	About ½ mile from last station.
4		2⅜	7	
5		3¼	4	
6	7 17	3¾	3	Changed course, running E.
7		3⅝	2	About ½ mile from last station.
8		3¾	1	" " "
9		3½	5	" " "
10	7 28	3⅜	7	Changed course, running N. by W. ½ W.
11		2½	8	
12		2½	5	About ½ mile from last station.
13		2	5	Changed course, running W.
14	7 50	2¼	5	
15		2⅞	3	
16		3⅛	2	
17	8 00	3¾	0	Sound lost.

On August 3d, I had an opportunity to hear this fog-signal myself, and to note its audibility. The wind was from the south and very light; the air was damp, smoky, hazy, and, as the sailors say, hung low; the barometer stood at 29.90; the tide was about flood. Our steamer was run for six miles in the axis of the siren's trumpet, which was sounded for our benefit at its full force. Note was made every third minute in a scale of ten of the intensity of the sound, and it was found that the audition decreased normally with the distance for the first two miles; at 2¼ miles it had fallen off one-half; at 3 miles it had fallen to one-tenth its power; at 3¼ miles away we could hear but a faint murmur, and when 4 miles distant, we had lost it completely; and yet there seemed to be no reason why we should not have heard it clearly at three times that distance.

The next morning was calm, but heavy with white fog; yet we heard the Little Gull siren distinctly though it was 10½ miles off, as we lay at our dock in New London. The steamer ran out of the harbor, but was compelled to anchor so thick was the fog; yet we heard Little Gull though 7½ miles off, at a force of 6 in the scale of ten, and the sound was so clear cut and distinct that we could differentiate it from the siren at the New London light, which was much nearer to us. The steamer worked round to inspect the neighboring lights, and we heard the Little Gull siren when at North Dumpling light station, 7 miles off, at a force of 6; at Morgan's Point Light, 10 miles off, at a force of 5, and we continued to hear it at an intensity of from 5 to 6 as we worked around among the other lights, within a compass of 10 miles, till the fog broke and the siren ceased.

Opportunity soon occurred for making more critical experiments. On a fine day we ran out to Little Gull, had the siren started under full steam, and then, following out a pre-arranged program, ran round Little Gull Island in such way, as to describe a rectangle of about 8 by 10 miles, its longest side running nearly north and south. No fixed rate of speed was maintained, but the steamer slowed, backed, or stopped, as was necessary. The atmosphere was what the sailors call lumpy, and Prof. Tyndall calls non-homogeneous. Prof. Henry, when writing of a like condition, said: *"As the heat of the sun increases during the first part of the day, the temperature of the land rises above that of the sea, and this excess of the temperature *produces upward currents of air*, disturbing the general flow of wind, both at the surface of the sea and at an elevation above." Observations were made and noted in a scale of ten, of the force or intensity of the signal's sound as it reached us at the end of each minute. The following Table D shows a sufficient number of the results for our purposes, taken from the tabulated schedule of our notes. The table also shows the condition of the atmosphere during our observations.

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*L. H. Board's Rep. for 1875, page 116.

Table D.

Observations at Little Gull Island, Long Island Sound, August 9, 1881, commencing at 10 A. M. Thermometer—Dry Bulb, 73°.09, Wet Bulb, 73° Fahr. Barometer, 29.77 Wind, S. W., force, 3. Cir. Strat. Clouds about the horizon.

Number of Observation.	Time of Observation.	Distance from Little Gull Island in statute miles.	Intensity of sound in scale of ten.	Number of Observation.	Time of Observation.	Distance from Little Gull Island in statute miles.	Intensity of sound in scale of ten.
	h. m.				h. m.		
1	10 30	0¼	10	16	12 04	2⅝	9
2	10 32	0⅓	10	17	12 08	2¼	9
3	10 34	0½	10	18	12 13	2⅛	5
4	10 36	1	10	19	12 20	2⅛	3
5	10 37	1¼	0	20	12 28	3¼	1
6	10 48	2	0	21	12 35	3½	0½
7	10 57	3	0	22	12 41	3⅜	0
8	11 02	3	0	23	12 45	3	1
9	11 08	3 1/16	1	24	12 57	2½	0
10	11 15	3½	3	25	12 58	2⅜	0
11	11 23	4⅛	4	26	1 02	1½	1
12	11 38		8	27	1 20	1¾	0½
13	11 42	2¼	9	28	1 24	1⅝	0½
14	11 54	3	9	29	1 30	0¾	0
15	11 57	3¼	9	30	1 32	0¼	10

At 4 P. M. two of us went in a row boat to Little Gull from the steamer which lay to her anchor half a mile off, and verified the fact that the fog-signal had been in full operation during the time of our observations by the report of the steamer's mate, who had been left there for that purpose. It then occurred to us to investigate still more closely what appeared to be a space—a circle of silence—in which we had, during the experiments of the morning, failed to hear the signal. After having had the siren put in full operation again, we pulled toward the nearer end of Great Gull Island, the siren sounding meantime with earsplitting force. When about 600 yards away we suddenly lost the sound as completely as if the signal had stopped. Pulling toward the steamer, not more than 200 yards, we reached a position at right angles with the axis of the siren's trumpet when we suddenly heard the sound again at its full force. Thus, in pulling 500 yards, we passed from complete audition of the signal to absolute inaudition; and then we passed back again to complete audition by pulling 200 yards in

another direction. All this took place within half an hour in open water, always in full view of the signal station, and without any visible obstacle being interposed or removed.

While on the island we learned that one of the light-house keepers, who had been on leave, had just returned from Sag Harbor, twenty miles away to the southeast. He had failed to hear the signal at all, until opposite the eastern end of Great Gull Island, and until he was within half a mile of the siren which was in full operation.

On the next morning our steamer anchored about a mile north of Little Gull; the wind was light, the air was clear, and the day was warm and beautiful. As it had been preceded by a warm night the atmosphere was homogeneous, and it was expected that we should have a day of normal audition and barren of curious phenomena. After the siren had commenced its noise we ran down to a point within half a mile of the light-house, and then steamed for Plum Island, running a little south of east for six miles, when we returned as nearly as might be on our own track. The results were curious. We lost half the force of the sound when within a quarter of a mile of the siren; a moment later we had lost four-fifths of it. Running another half mile we were off the middle of Great Gull Island, and the sound had increased to a force of four; in five minutes more it had dropped to three; from that on, until we reached the end of our six mile run, it gradually weakened, and it had dropped to a force of two when we turned and ran back to our anchorage. It is particularly curious that the sound had the same intensity at three-sixteenths of a mile from its source, and at six whole miles from that point, while it varied from two to ten in a scale of ten between those points. The results of the trip are more fully and exactly given in Table E.

Thinking that possibly this peculiarity might have been induced by those differences of temperature in the strata of the atmosphere suggested by Dr. Tyndall as probable cause for such phenomena, effort was made to ascertain something of these differences by sending a thermometer to the upper air. In the course of the afternoon we made a kite some six feet high, attached to it a self-registering thermometer, and after a number of trials succeeded in getting it up about five hundred feet, and in hauling it safely in again after it had been up over an hour. The thermometer had a wet bulb, and beside was protected from the direct rays of the sun; but it, registered only half a degree more of heat at its highest point than it had done in the pilot-house. The course the kite took showed no difference between the air currents alow and aloft.

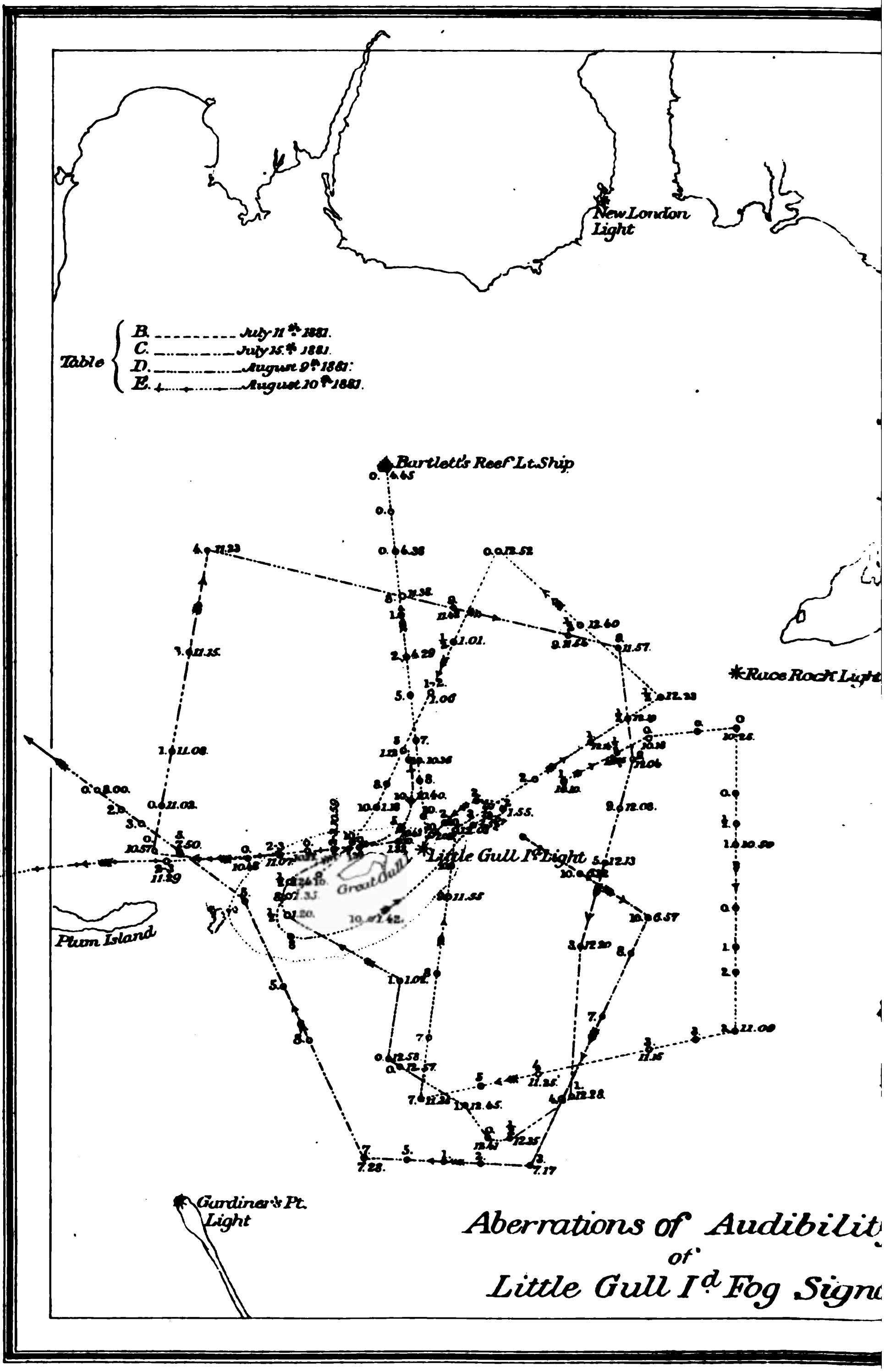
Table E.

Observations at Little Gull Island, Long Island Sound, August 10, 1881, commencing at 10:30 A. M. Dry Bulb Thermometer, 76°, Wet Bulb, 75°. Barometer, 29.40. Wind, W. by N., force 3, and steady throughout. Day clear and beautiful.

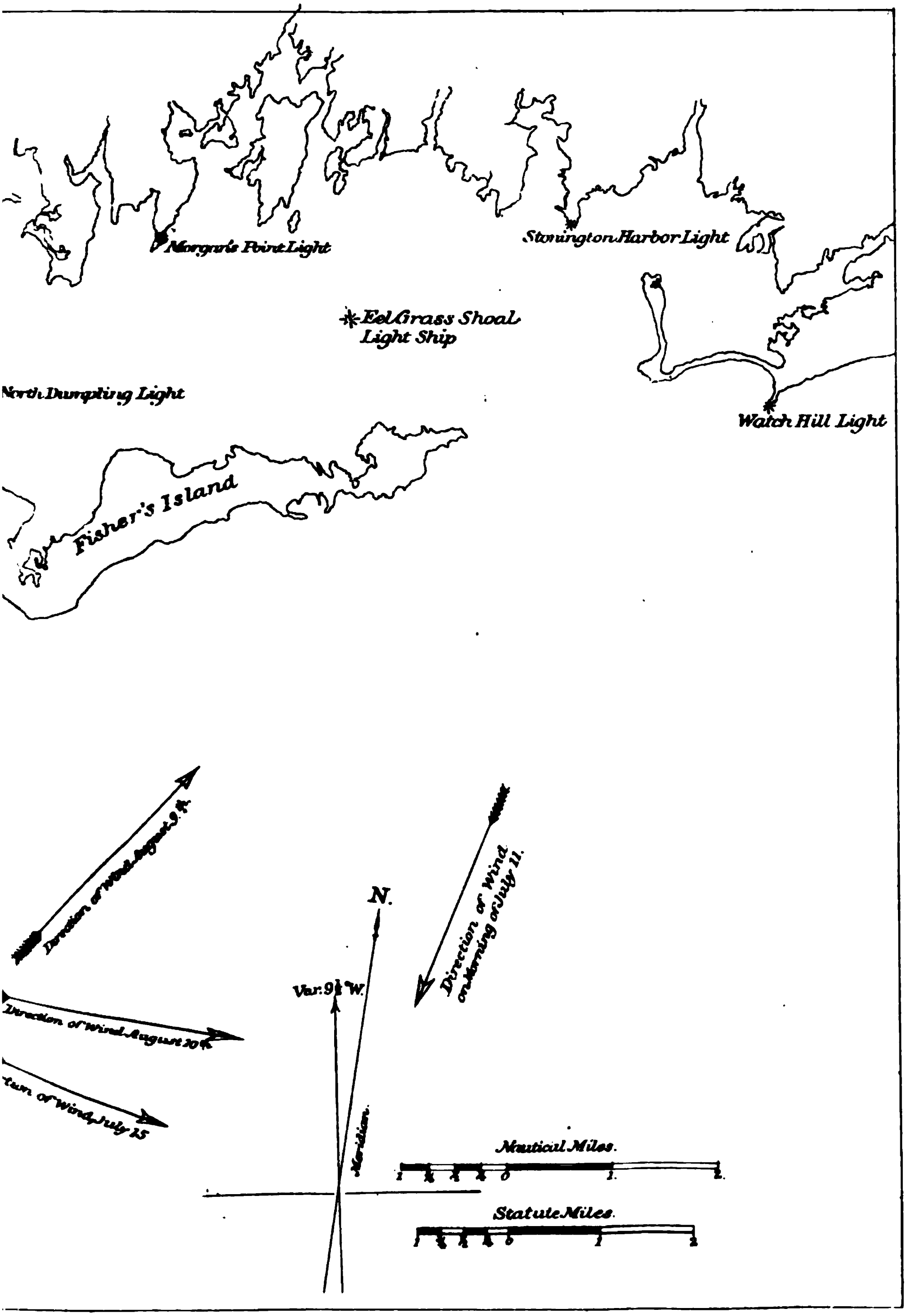
Number of Observation.	Time of Observation.	Distance from Little Gull Island in a direct line in statute miles.	Intensity of sound in a scale of ten.	Number of Observation.	Time of Observation.	Distance from Little Gull Island in a direct line in statute miles.	Intensity of sound in a scale of ten.
	h. m.				*h. m.*		
1	10 36	$1\frac{1}{16}$	10	7	10 59	$2\frac{1}{16}$	2 to 3
2	10 40	$0\frac{3}{4}$	10	8	11 07	$2\frac{1}{2}$	2 to 3
3	10 44	$0\frac{1}{4}$	5	9	11 29	$2\frac{5}{8}$	2 to 3
4	10 45	$0\frac{3}{16}$	2	10	11 45	$5\frac{1}{16}$	2 to 3
5	10 49	$0\frac{3}{4}$	4	11	11 52	$5\frac{1}{2}$	2
6	10 53	$1\frac{1}{4}$	3	12	12 02	6	2

The Light House Board has known from the first that aberrations in audibility might occur near any fog-signal. When the fog-trumpet was set up at Beaver Tail Point in 1856, the Naval Secretary of the Board, then Lieutenant, now Rear Admiral Jenkins, U. S. N., in company with Mr. Daboll, its inventor, found, in returning to Newport, that they lost the sound of the signal between Beaver Tail and Fort Adams, and recovered it again between the Fort and Newport, as did later observers, and that this failure to hear it did not result from any failure of the signal to operate.

The Board's publications show that Prof. Henry, its scientific adviser, had the subject for many years continuously under advisement, and that between 1865 and 1878, many experiments were made, and various reports on them were submitted to the Board, as to the use and value of its several kinds of fog-signals. In 1870 the Board directed General Duane, of the U. S. Engineers, then and still in its service, to make a series of experiments to ascertain the comparative value of its different signals. In his report the General said, speaking of the steam fog-signals on the coast of Maine:



New London Light
Table
B. July 11th 1881.
C. July 15th 1881.
D. August 9th 1881.
E. August 10th 1881.
Bartlett's Reef Lt. Ship
Race Rock Light
Little Gull Id Light
Great Gull
Plum Island
Gardiner's Pt. Light
Aberrations of Audibility
of
Little Gull Id Fog Signal


Morgan's Point Light
Stonington Harbor Light
Eel Grass Shoal
Light Ship
North Dumpling Light
Watch Hill Light
Fisher's Island
Direction of Wind August 9th.
Direction of Wind on Morning of July 11.
N.
Var. 9½ W.
Direction of Wind August 10th.
Direction of Wind July 25
Meridian
Nautical Miles.
Statute Miles.

*"There are six steam fog-whistles on the coast of Maine; there have been frequently heard at a distance of twenty miles, and as frequently cannot be heard at the distance of two miles, and this with no perceptible difference in the state of the atmosphere.

"The signal is often heard at a great distance in one direction, while in another it will be scarcely audible at the distance of a mile. This is not the effect of wind, as the signal is frequently heard much farther against the wind than with it; for example, the whistle on Cape Elizabeth can always be distinctly heard in Portland, a distance of nine miles, during a heavy northeast snow-storm the wind blowing a gale directly from Portland toward the whistle."

* * * * * * * * * *

"The most perplexing difficulty, however, arises from the fact that the signal often appears to be surrounded by a belt, varying in radius from one to one and a half miles, from which the sound appears to be entirely absent. Thus, in moving directly from a station, the sound is audible for the distance of a mile, is then lost for about the same distance, after which it is again distinctly heard for a long time. This action is common to all ear-signals, and has been at times observed at all the stations, at one of which the signal is situated on a bare rock twenty miles from the main land, with no surrounding objects to affect the sound."

Prof. Henry, in considering the results of Gen. Duane's experiments, and his own, some of which were made in company with Sir Fred'k Arrow and Capt. Webb, H. B. M. Navy, both of the British Light-House Establishment, who were sent here to study and report on our fog-signal system, formulated these abnormal phenomena. He said they consisted of:

"1. The audibility of a sound at a distance and its inaudibility nearer the source of sound.

"2. The inaudibility of a sound at a given distance in one direction, while a lesser sound is heard at the same distance in another direction.

"3. The audibility at one time at a distance of several miles, while at another the sound cannot be heard at more than a fifth of the same distance.

"4. While the sound is generally heard further with the wind than against it, in some instances the reverse is the case.

"5. The sudden loss of a sound in passing from one locality to another in the same vicinity, the distance from the source of sound being the same." †

These experiments were not confined to our own shores. Dr. Tyndall, the well known English physicist, who stands in the same relation to the British Light-House Establishment that Prof. Henry did to our own, writes thus:

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*Annual Rep't L. H. Board 1874, pp. 99-100.

† L. H. B. Annual Rep. 1875, page 106.

"With a view to the protection of life and property at sea, in the years 1873 and 1874, this subject received an exhaustive examination, observational and experimental. The investigation was conducted at the expense of the Government, and under the auspices of the Elder Brethren of the Trinity House [the governing body of the British Light-House Establishment.]

"The most conflicting results were at first obtained. On the 19th of May, 1873, the sound range was 3½ miles; on the 20th it was 5½ miles; on the 2d of June 6 miles; on the 3d more than 9 miles; on the 10th 9 miles; on the 25th 6 miles; on the 26th 9¼ miles; on the 1st of July 12¾ miles; on the 2d 4 miles, while on the 3d, with a clear, calm atmosphere and smooth sea, it was less than 3 miles." *

The officer who made the reports, as to the fog-signals at Beaver Tail and Little Gull, after the accidents to the steamers Rhode Island and Galatea heretofore mentioned, was the Assistant Inspector of the Third Light-House District, Lieut. Comd'r F. E. Chadwick, U. S. N.; and it was he who had charge of the Light-House steamer while the foregoing observations were being made, after Capt. George Brown, U. S. N., the Inspector—to whom I am indebted for many courtesies on this trip—was called elsewhere by other official duties. Mr. Chadwick brought to this work an unbiased mind, trained in the severest schools of scientific investigation. His object in all his experiments was simply to ascertain the exact truth for practical official purposes. He had not proposed, even to himself, to make any generalizations from his observations. But he kindly answered certain of my questions as to the opinions which had forced themselves upon him, and his answers are here set down for the consideration of those who use these fog-signals overmuch as a guide for their ships.

"It seems to me" he said "that navigators should understand that when attempting to pick up a fog-signal attention must be given to the direction of the wind, and that if they are to windward, (in a moderate breeze,) the chances are very largely against hearing it, unless close to; that there is nearly always a sector of about 120° to windward of the signal in which it either cannot be heard at all, or in which it is but faintly heard. Thus, with the wind E. S. E., so long as they are bearing from the signal between N. E. and South, there is a large chance that the signal will not be audible until it is very close.

"As they bring the signal to bear at right angles with the wind, the sound will almost certainly in the case of light wind increase, and it will soon assume its normal volume—being heard almost without fail in the leeward semicircle.

"Fog, to my mind, and so far as my experience goes, is not a factor of any consequence whatever in the question of sound. Signals may be heard at great dis-

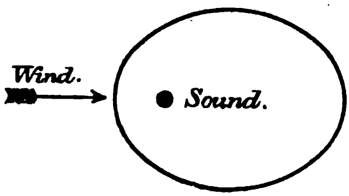
*Sound, by Tyndall, 3d Edition English, page 324.

tances through the densest fogs, which may be totally inaudible in the same directions and at the same distances in the clearest atmosphere. It is not meant by this last statement that the fog may assist the sound; as at another time the signal may be absolutely inaudible in a fog of like density, where it had before been clearly heard. That fog has no great effect can easily be understood when it is known, (as it certainly is known by observers,) that even snow does not deaden sound—there being no condition of the atmosphere so favorable for the far reaching of sound signals as is that of a heavy N. E. snow storm, due supposably to the homogeneity produced by the falling snow.

"It seems to be well established by numerous observations that on our own northern Atlantic coasts the best possible circumstances for hearing a fog-signal are in a northeast snow storm, and, so far as these observations have extended, they seem to point to the extraordinary conclusion that they are best heard with the observer to windward of the signal; and that in light winds the signal is best heard down the wind, or at right angles with the wind.

"The worst conditions for hearing sound seem to be found in the atmosphere of a clear, frosty morning on which a warm sun has risen and has been shining for two or three hours.

"The curve of audibility in a light or moderate breeze, in general, is similar to that plotted by Prof. Henry, as in the accompanying diagram.



"I think it is established that there are two great causes for these phenomena, non homogeneity of the atmosphere, and the movement of the wind; how this latter acts no one can say. The theory of retardation of the lower strata of the atmosphere near the earth's surface, as advanced by Prof. Stokes, of England,* seems good for moderate winds, but it hardly holds in cases where the siren is heard from eighteen to twenty miles to windward during N. E. gales."

While the mariner may usually expect to hear the sound of the average fog-signal normally as to force and place, he should be prepared for occasional aberrations in audition. It is impossible at this point in the investigations which are still in progress, to say when, where or how the phenomena will occur. But certain suggestions present themselves even now as worthy of consideration.

It seems that the mariner should, in order to pick up the sound of the fog-signal most quickly when approaching it from the wind-

*See Henry on Sound, p. 533; or, Sm. Rept., 1878, p. 533; or, L.-H. B. Rept. for 1875, p. 120. See Henry on Sound, p. 512, and Taylor in Am. Jour. Sci., 3d series, VXI, p. 100; also, Rept. Brit. Assoc., XXIV, 2d part, p. 27.

ward, go aloft; and that, when approaching it from the leeward, the nearer he can get to the surface of the water the sooner he will hear the sound.

It also appears that there are some things the mariner should not do.

He should place no negative dependence on the fog-signal; that is, he should not assume that he is out of hearing distance because he fails to hear its sound.

He should not assume that, because he hears a fog-signal faintly, he is at a great distance from it.

Neither should he assume that he is near to it because he hears the sound plainly.

He should not assume that he has reached a given point on his course because he hears the fog-signal at the same intensity that he did when formerly at that point.

Neither should he assume that he has not reached this point because he fails to hear the fog-signal as loudly as before, or because he does not hear it at all.

He should not assume that the fog-signal has ceased sounding because he fails to hear it even when within easy earshot.

He should not assume that the aberrations of audibility which pertain to any one fog-signal pertain to any other fog-signal.

He should not expect to hear a fog-signal as well when the upper and lower currents of air run in different directions; that is when his upper sails fill and his lower sails flap; nor when his lower sails fill and his upper sails flap.

He should not expect to hear the fog-signal so well when between him and it is a swiftly flowing stream, especially when the tide and wind run in opposite directions.

He should not expect to hear it well during a time of electric disturbance.

He should not expect to hear a fog-signal well when the sound must reach him over land, as over a point or an island.

And, when there is a bluff behind the fog-signal, he should be prepared for irregular intervals in audition, such as might be produced could the sound ricochet from the trumpet, as a ball would from a cannon; that is, he might hear it at 2, 4, 6, 8 and 10 miles from the signal, and lose it at 1, 3, 5, 7, 9 and 11 miles distance, or at any other combination of distances, regular or irregular.

These deductions, some made, as previously mentioned, by several of the first physicists of the age, and some drawn from the original investigations here noted, are submitted for consideration rather than given as directions. They are assumed as good working hypotheses for use in further investigation. While it is claimed that they are correct as to the localities in which they were made, it seems proper to say that they have not been disproved by the practical mariners who have given them some personal consideration, and who have tried to carry them into general application. Hence these suggestions have been set down in the hope that others with greater knowledge and larger leisure may give the subject fuller attention, and work out further results.

If the law of these aberrations in audibility can be evolved and some method discovered for their correction, as the variations of the compass are corrected, then sound may be depended upon as a more definite and accurate aid to navigation. Until then, the mariner will do well when he does not get the expected sound of a fog-signal, to assume that he may not hear a warning that is faithfully given, and then to heave his lead, and resort to the other means used by the careful navigator to make sure of his position.

Mr. CLEVELAND ABBE remarked that it seemed to him if these anomalies were due to the refraction of sound in a vertical plane, then a few feet of increase in the altitude of the observer or of the signal itself, would make a great difference in the result. To this Mr. JOHNSON replied that the observations made on board the vessels were attended with the same results as to degree of audibility, whether the observer were stationed upon the mast, deck, or near the water line of the vessel.

Mr. WILLIAM B. TAYLOR said that the interesting observations presented by Mr. Johnson were in the main entirely corroborative of the results announced by our late President, Prof. Henry; and the anomalies noted furnished striking confirmation of the explanations and generalizations reached by him, while they as strikingly discredited as incongruous the rival hypothesis of hygroscopic flocculence in the atmosphere as a notable occasion of acoustic disturbance. When we consider the wide areas over which fog-signals are designed to be conveyed—through which spaces the atmosphere can rarely be uniform, either in its temperature or its movements—we can readily understand that from these two prominent conditions of sound-refraction, acoustic rays are commonly propagated in quite sensibly curved or often serpentine directions; and that while these inequalities will sometimes favor audibility at given points, they will as often impair or defeat it. Moreover, these deformations of sound waves are not confined to vertical planes, since it has been shown that *lateral* refractions may exist, giving false impressions of direction as well as of distance.

As we have no means of either controlling or accurately determining these simultaneous differences of wind and temperature, we are forced to admit that the practical difficulties attending these anomalies of sound propagation are insoluble and incurable. But we must not hence abandon sound-signalling as either hopeless or inefficient, since it is the best—or rather the only—method at our disposal of giving warning and guidance to the befogged mariner.

Two partial alleviations of the recognized defects are suggested. The first is to place the siren or the steam whistle at considerable elevations, say on the top of skeleton towers, perhaps higher than those ordinarily employed as light-towers; at which points they could readily be operated from the ground. This would, in many cases, counteract the tendency to local acoustic shadows or bands of silence, though in other cases it would be quite ineffectual. The second expedient is, (if not too expensive,) to greatly multiply the number of such signals at available points about dangerous coasts or inlets, with proper distinctions to clearly specialize their indications, in order that the mariner failing to catch the sound from one direction, might have the probability of picking up the sound from a different azimuth. As these sound instruments may be operated at considerable distances from the engine, and even at practically inaccessible positions, on rocks or on buoys, *danger points* especially should be guarded by fog-signals, not necessarily of great power, but capable, at least, of covering the radius of actual insecurity.

Remarks were made by Mr. WILLIAM B. TAYLOR on the relation of fog and snow storms to audibility.

With regard to fog, Mr. TAYLOR said, we are not to conceive the sound vibrations as passing alternately through air and water, (as a ray of light does,) but taking into view the average wave-length of sound (several feet ordinarily) and the enormous number of water particles contained in that space, we must contemplate the whole mass as a homogeneous medium taking up the sound waves in the same manner, whether the air were perfectly dry, or were precipitating excessive moisture in the form of rain. In the absence of sensible wind, the air thus supersaturated with moisture would be practically very homogeneous, and thus generally well adapted to the normal transmission of sound.

A similar remark applies to falling snow, (when not accompanied with strong wind,) with the additional circumstance that, while the precipitation and congelation would tend to warm the upper regions of the air, any melting of the snow as it fell would cool the lower region. This condition of relative warmth above and cold below is favorable to the conveyance of sound to a distance—as first pointed out by Prof. Osborn Reynolds, of Manchester,—by reason of the expanding spherical wave-front being slightly more accelerated above than below, (in accordance with well known principles,) and thus causing the horizontal or slightly rising sheets of sound to be dished downward.

The next communication was by Mr. WILLIAM HARKNESS on the relative accuracy of different methods of determining the solar parallax.

This paper is published in full in the *American Journal of Science* for November, 1881, No. 131, vol. 22, pp. 375–394.

205TH MEETING. NOVEMBER 5, 1881.

The President in the Chair.

Forty-three members present.

MR. J. C. WELLING presented the following communication on

ANOMALIES OF SOUND SIGNALS.

In the year 1865 Prof. Henry, while making some observations on the intensity of sounds, discovered that a sound moving against the wind, and which was inaudible to the ear of an observer on the deck of a vessel, might sometimes be regained by ascending to the mast-head; that is, sound is sometimes more readily conveyed by the upper current of the air than by the lower.

This fact, with other corroborative facts, did not, he says, reveal its full significance to him until he was able to interpret it by the aid of the hypothesis of Prof. Stokes. (Transactions of the British Scientific Association for 1867, Vol. 24,) according to which there is—when the wind blows—a difference of velocities between the upper and the lower strata of the atmosphere, resulting from the retardation of the lower stratum by friction with the ground. This unequal movement of the atmosphere disturbs the spherical form of the sound waves, and tends to make them somewhat of the form of an ellipsoid, the section of which by a vertical diametral plane, parallel to the direction of the wind, is an ellipse, meeting the ground at an obtuse angle on the side towards which the wind is blowing, and at an acute angle on the opposite side. But as sound moves in a direction perpendicular to the front of the sound waves, it follows that sounds moving with a favorable wind tend to be tilted downwards toward the ground; and sounds moving against an opposing wind tend to be tilted upward until, finally, they pass above the head of a listener standing on the ground.

The effect of different elevations on the audibility of the same sound has been brought within the sphere of scientific experiment. In some experiments made by Prof. Reynolds in 1874, on "a flat meadow," by the aid of an electrical bell, placed one foot from the ground, it was found that elevation affected the range of sound against the wind "in a much more marked manner than at right angles." He adds: "Over the grass no sound could be heard with the head on the ground at twenty yards from the bell, and at thirty yards it was lost with the head three feet from the ground, and its full intensity was lost when standing erect at thirty yards. At seventy yards, when standing erect, the sound was lost at long intervals, and was only faintly heard even then; but it became continuous again when the ear was raised nine feet from the ground, and it reached its full intensity at an elevation of twelve feet."*

In some experiments made by Prof. Henry, in 1875, he found that while sound moving at right angles to the wind could not be heard as far as sound moving with the wind, yet it was equally true

* London, Ed., and Dub. Ph. Mag. for 1875, Vol. 50.

of sounds moving against the wind and at right angles to the wind, that they could both be better heard on the top of a high tower than on the surface of the ground.*

Baron Humboldt, in observations made on the intensity of sounds at the Falls of the Orinoco, remarked their greater audibility by night than by day, and referred their comparative weakness by day to the effect of atmospheric disturbances arising from ascending currents of rarified air and descending currents of heavier air, which broke up the homogeneity of the atmosphere, and thereby obstructed the transmission of sound. It is a necessary complement of this hypothesis that sound which fails to be transmitted through the atmosphere, because of "the reflections which it endures at the limiting surfaces of the rarer and the denser air," is liable to be returned to the hearer in the shape of aerial echoes rebounding from the acoustic cloud which the primary sound is not able to pierce; and hence the logical place assigned to echoes by Dr. Tyndall, when, adopting and applying the Humboldt hypothesis, he says that "rightly interpreted and followed out, these aerial echoes lead to a solution which penetrates and reconciles the phenomena from beginning to end." "On this point," he says, "I would stake the issue of the whole inquiry. * * * The echoes afford the easiest access to the core of this question." †

The conflicting hypotheses of Humboldt and Stokes, as respectively applied by Tyndall and Henry in interpreting the abnormal phenomena of sound, are here cited as prefatory to some much older observations made under the same head by Dr. W. Derham, in his elaborate paper entitled "Experiments and Observations on the *Motion of Sound*, and other things pertaining thereto," as read before the Royal Society in 1708. This paper, written in Latin, is the report of a systematic inquiry into phenomena pertaining to the velocity and motion of sounds, and treats only incidentally on the intensity of sounds; but, nevertheless, it contains some interesting statements under this latter head.‡

The subject of echoes is the first which engages the writer's attention. He says that echoes produced by sound-reflecting objects situated near a sounding body may sometimes be heard through many

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* Rep. of Light-House Board, 1875, p. 119.

† "Sound," p. xxiv.

‡ Phil. Trans. of Royal Society, Jan. and Feb., 1708.

miles, as well as the primary sound, or even better than the latter. He observes that echoes produced by the firing of cannon on the Thames river, between Deptford and Cuckold's Point, came to his ears in a multiple form, repeated five or six times, and the terminal crash of the echo was the loudest. This last feature was observed even when the multiple sounds were nine or ten in number. To this he adds: "When I have heard the crashes of heavy artillery, especially in a still and clear atmosphere, I have often observed that a *murmur* high in the air preceded the report. And in thin fog I have often heard the sound of cannon running in the air, high above my head, through many miles, so that this murmur has lasted fifteen seconds. This continuous murmur, in my opinion, comes from particles of vapor suspended in the atmosphere which resist the course of the sound waves, and reverberate them back to the ears of the observer after the manner of undefined echoes.*

Mr. Richard Townley, an intelligent observer, having written to Dr. Derham, in a letter from Rome, that "sounds are rarely heard as far at Rome as in England and in other northern regions, and having cited in support of this statement some observations drawn from the firing of cannon in the castle of St. Angelo, Dr. Derham caused an enquiry on this point to be made in Italy, under the auspices of the British Minister at Florence. The enquiry was conducted by Joseph Averani, a Professor in the University of Pisa. Guns were fired at Florence, and observers were stationed at different points in Leghorn and its vicinity to mark the effect of the reports. The observers stationed in the Light-House and the Marzocco tower, in the lower part of the city, heard no reports, but observers stationed on an old fortress in the upper part of the city, and other observers placed on Monte Rotondo, about five miles from Leghorn in the direction of Mount Nero, (and, therefore, more in the direction of the wind which was blowing across the path of the sound,) were able to hear the reports.

Another series of experiments was made on water, by firing cannon at Leghorn, and stationing observers at Porto Ferrajo in the Island of Elba, a distance of about sixty miles. In this case the reports were better heard in still air than when the wind was either favorable or unfavorable, and were not heard at all points equally well, but only at those which were a little the more elevated.†

* Derham, p. 10. † *Ibid.*, pp. 18, 19, 20.

As to the result of these observations, it was easy for Dr. Derham to conclude that sounds are heard as far in Italy as in England, when the conditions of the atmosphere are the same; and these experiments are here cited only for the light they shed on the comparative antiquity of the observation that elevation has an important bearing on the audibility of sounds.

As to the causes which really affect the intensity of sounds, Dr. Derham seems to have had a very obscure and imperfect notion. His observations under this head are mainly a bundle of contradictions, and the causes of these variations he prudently leaves to be investigated by others, seeing, as he says, "that it equally exceeds the grasp of his mind to discover them, and to assign what may be the proper medium or vehicle of sound." He does not, however, fall into the error of measuring the acoustic transparency of the atmosphere by its optic transparency, for he says that the clearest day he can remember, when wind and everything else seemed to concur in promoting the force and velocity of sound, was a day when he could not hear the firing of cannon at a distance easily penetrated by their reports on former occasions. The effect of clear or foggy air on sound, he says, is very uncertain, but as to *thick fogs* and snow, he affirms that they are certainly powerful dampers of sound, an observation now abundantly proved to be erroneous.

From some observations made by Gen. Duane, at Portland, Maine, in 1871, it appears that the fog-signal at that point is often surrounded by a belt of silence, varying from one to one and a half miles in radius.

From some observations made by Prony, Mathieus, and Arago, at Villejuif, and by Humboldt, Bouvard, and Gay-Lussac, at Montlhéry, in France, the two towns being 11.6 miles from each other, it was noticed that while every report of the cannon fired at Montlhéry was heard with the greatest distinctness, nearly every report from Villejuif failed to reach Montlhéry. The air at the time was calm, with a slight movement of wind from Villejuif toward Montlhéry, or "against the direction in which the sound was best heard." These observations were made in 1822.

In 1872, Prof. Henry observed the same non-reciprocity of sound in approaching the Whitehead fog-signal on the coast of Maine. At a distance of six miles the signal was heard; at a distance of three miles from the shore the sound of the signal was lost, and was not regained until the vessel approached within a quarter of a mile of the station. During all this time of silence the sound of the steamer's whistle was distinctly heard at the Whitehead station; that is, a lesser sound was heard from the steamer to the station, "while a sound of greater volume was unheard in the opposite direction." The wind at the time was blowing in favor of the steamer's whistle, and against the fog-signal.*

In a paper presented to the Royal Society in 1874, Prof. Reynolds showed that the form of the sound-wave is liable to flexure from changes in the temperature of the atmosphere as well as from the unequal motion of wind.†

These abnormal phenomena of sound, considered in connection with the hypothesis of Prof. Stokes, as enlarged and applied by Prof. Henry, may be reduced into the following generalizations which, if accurate in point of logical form, and true in point of the facts to which they are applied, may be stated under the guise of aphorisms, as follows:

1. "Where the condition of the air is nearest that of a calm, the larger will be the curve of audition, and the nearer will the shape of the curve approach to a circle, of which the point of origin of the sound, or the point of perception will be the centre." [This aphorism is stated abstractly from any consideration of temperature refraction which, so far as it exists, will always tend to modify the shape of the curve of audition.]‡

2. Apart from all consideration of temperature refraction, a sound will be heard furthest in the direction of a gentle wind, because the portion of the sound-wave thrown down from above, in this case, is re-enforced by the sound reflected from the surface, and will thus more than compensate for the loss by friction.||

3. Other things being equal, the area of audition will be proportionally diminished in the case of sounds moving against winds more or less strong, because the sonorous waves will be refracted above the ears of the observer. (Stokes, Henry and Reynolds.)

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* Rep. Light House Board, 1874, p. 108.

† London, Ed., and Dublin Phil. Mag. for 1875, Vol. 50, p. 52.

‡ Light-House Report for 1875, p. 125.

|| *Ibidem.* Cf., Tyndall's Sound, p. 311. Cf., Reynolds in Lon., Ed., and Dub. Ph. Mag. for 1875, Vol. 50, pp. 63, 68.

4. The area of audition will be diminished in the case of a sound moving with an overstrong favoring wind, because the sound-waves in this case will be so rapidly and strongly thrown down to the ground that the intensity of the sound will suffer more diminution from absorption and friction than can be supplied by the upward reflection of the sound rays conspiring with the gradual downward flexure of the sound-waves, as in the case of a gentle favoring wind.*

5. Sounds moving *against* a gentle wind will, *cæteris paribus*, be heard further than similar sounds moving *with* an overstrong favoring wind, for reasons already implied, because the downward flexure of the sound-waves, being excessive in the latter case, tends to extinguish the conditions of audibility more rapidly than is done by the slight upward refraction in the former case.

6. When sounds moving against the wind are heard further than similar sounds moving with a wind of equal strength, it is because of a dominant upper wind blowing at the time in a direction opposite to that at the surface.†

7. A sound moving against the wind, and so refracted as in the end to be thrown above the head of the observer will, at the point of its elevation, leave an acoustic shadow. But this acoustic shadow, at a still further stage, may be filled in by the lateral spread of the sound-waves, or may be extinguished by the downward flexure of the sound waves, resulting from an upper current of wind moving in an opposite direction to that at the surface, or resulting in a less degree from an upper stratum of still air. Under these circumstances, there will be areas of silence enclosed within areas of audition.‡

8. As sounds may be refracted either by wind, or by changing temperatures, or by both combined, it follows that, under many circumstances, a sound lost at one elevation may be regained at a higher elevation.‖

9. As sounds moving against the wind are liable to become inaudible (by being tilted over the head of the observer) even before

* Light-House Report, 1875, p. 125.

† Light-House Report for 1877: Experiments on Sound, p. 13.

‡ Experiments on Sound, 1877, p. 8.

‖ Henry and Reynolds. Cf., Delaroche, Ann. de Chim., 1816, Tome I, p. 180.

their intensity has been extinguished, we may find in this fact an explanation of the statement made by Reynolds, that "on all occasions the effect of wind seems to be rather against distance than distinctness."*

10. As sounds may be inaudible at certain distances and elevations without being wholly extinguished, it follows that the comparative inaudibility of sounds at different times cannot always be cited as an evidence of their relative intensities. The comparative inaudibility may be a function of variable refraction rather than of variable intensity. Hence the law of inverse squares, though perfectly true in its theoretical application to the measurement of the intensity of all sounds, cannot always be legitimately used to calculate backwards from the audibility of a sound, as empirically ascertained at a given point and elevation, to its relative intensity as previously heard at the same point and elevation.

11. The hypothesis of Stokes, as applied by Henry, does not exclude the hypothesis of Humboldt, but reduces the latter to a very subordinate and inappreciable place in interpreting the abnormal phenomena of sound.

12. The hypothesis of Stokes, as applied by Henry, does not exclude the reasoning or the experimental proofs by which Prof. Reynolds demonstrates that differences in temperature exert a refracting power in sound, but finds in that refraction an influence which may sometimes accelerate and sometimes retard the refraction produced by wind.†

The next communication was by Mr. C. H. KOYL, Fellow of the Johns Hopkins University, on

THE STORAGE OF ELECTRIC ENERGY.

After discussing the subject from an historical point of view, concluding with a description of the improved form of secondary battery lately invented by M. Faure, the author proceeded to state the

* Lon., Ed., and Dub. Ph. Mag. for 1875, Vol. 50, p. 63.

† Rep. Light-House Board 1875, p. 125, cf. Reynolds; Lon., Ed., and Dub. Ph. Mag. for 1875, Vol. 50, p. 71.

results of some investigations carried on independently in this country by Mr. J. A. Maloney and Mr. Franz Burger, of Washington, and afterward by himself in connection with them.

Mr. Maloney and Mr. Burger had been aiming to interpose in the circuit of the electric lamp a reservoir of energy which should perform the same function for the electric lamp that a gasometer did for a gas-burner, viz., prevent its flickering by keeping a constant or nearly constant potential on the main line, even though the current from the source should be irregular.

A long course of experiment convinced them that plates of lead immersed in dilute sulphuric acid form a combination preferable to any other for giving return currents when once these plates have been made part of an electric circuit. They noticed what they believed to be an oxide of lead formed on one plate, and since the thicker the coating of oxide the greater the effect, they began to regard this layer as a sort of sponge which, in some way, held the electricity, and they concluded to increase the holding capacity of the cell by increasing the thickness of the sponge. Oxide of lead was accordingly purchased and painted on, with results which were surprising. The storage of electricity in large quantity was effected. This was of course independent and without any knowledge of Mr. Faure's work in Europe, but the chief merit of their inquiry lies in the rapidity with which they grasped the idea of *mechanically* increasing the sponge-like coating.

While they were testing the capabilities of the battery and were still endeavoring to improve it, the announcement was made of Mr. Faure's similar inventions. Soon after the battery was submitted for experiment to three members of this Society, and subsequently the co-operation of the author was invited for further study of the subject.

On examining the plates during their summer investigations they found reason for believing that the published theory of the action of the cell was but partly correct; for after the plates had been charged the changes of color and, therefore, of chemical constitution, upon which the return current was supposed to depend, were found, in general, not to take place until the return current had been passing for some time. If so, in something else than chemical combination must lie the storage capacity of these cells. The conclusion arrived at from their investigations was that the change of red-lead into peroxide upon one plate and into spongy lead upon the other required only a small part of the oxygen and hydrogen liberated by the primary current and that the remainder was mechanically held in the coatings.

Several minor considerations support this view, and the principal experiments upon which the proof should rest, viz., the liberation of the gas in a vacuum or by slight application of heat in general succeed. Some anomalies, however, are presented which require further study, but which the author hopes soon to reconcile with the theory of mechanical storing.

A discussion followed, in which several members participated.

206TH MEETING. NOVEMBER 17, 1881.

The President in the chair.

Thirty-eight members present.

The communication for the evening was by Mr. G. K. GILBERT

ON BAROMETRIC HYPSOMETRY.

This communication was reserved by the author, and his views and investigations in connection with this subject will be found in a paper contributed by him to the Second Annual Report of the Director of the United States Geological Survey.

A brief discussion ensued, and one or two points were questioned

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207TH MEETING. DECEMBER 3, 1881.

The President in the chair.

Seventy-six members and visitors present.

Under the rules this meeting, being the next preceding the annual meeting, was set apart for the delivery of the address of the retiring President of the Society. Calling Vice-President Hilgard to the chair, the President of the Society, Mr. J. J. WOODWARD, then read the following address:

MODERN PHILOSOPHICAL CONCEPTIONS OF LIFE.

I address you this evening in accordance with the fifth of the new Standing Rules for the government of the Philosophical Society of Washington, adopted in January last, which directs that the stated meeting next preceding the annual meeting for the election of officers shall be set apart for the delivery of the President's Annual Address. By the rules adopted at the first organization of the society the President's address was directed to be delivered on the evening of the annual meeting after the election of officers had taken place. It was found, however, that the elections always occupied the whole meeting, so that the address was necessarily postponed until after the term of office for which the President was elected had expired. During the presidency of the illustrious Professor Henry, who by common consent was re-elected annually, the inconvenience of this arrangement was not felt. But I understood the general sense of the Society last year to be that an annual change of President is desirable, and that this standing rule was adopted in view of that feeling, in order to give the retiring President a convenient opportunity for the delivery of his address before his term of office expires.

For my own part I was last year, and am now, thoroughly convinced of the desirability of electing a new President annually in a society like ours. I think on the one hand that it is a measure well calculated to increase the interest taken in the society by its members, and on the other hand that the preparation of a formal annual address would be too great a tax upon the time of a President re-elected from year to year. I think, too, that there is much propriety in a suggestion which I heard expressed in many quarters last year, that our President should be selected alternately, from what may be called for convenience, the Physical and Biological sides of the society, so that having been myself elected as in some sort a representative of the Biological side, it is my hope that you will at the next meeting elect as my successor a representative of the Physical side. With this brief explanation I will proceed at once to the consideration of the subject I have selected for the present occasion.

I propose to invite your attention this evening to some thoughts on the *Modern Philosophical Conceptions of Life.* The theme is so large that it would be idle to attempt its systematic treatment in the course of a single evening; nor do I pretend to be in possession of any satisfactory solution of this ancient question, of which I might offer you an abstract or outline, pending the fuller presentation of my results elsewhere. Yet I have ventured to hope that a discussion of some of the considerations involved, and a brief statement of certain views that I have been led to entertain, would not be without interest, and perhaps might prove of actual service, especially to those of you who are engaged in biological pursuits.

Undoubtedly the conception of life most popular at the present time is that which assumes all the phenomena of living beings to be the necessary results of the chemical and physical forces of the universe, and claims, or intimates, that wherever this has not yet been proven to be the case the evidence will hereafter be forthcoming. This doctrine, which may conveniently be designated the chemico-physical hypothesis of life, has readily found its way from the speculative writings of philosophers to the rostrums of some of our teachers of chemistry and physics who boldly declare, in their class-lectures and public addresses, that the forces at work in the inorganic world are fully adequate to explain all the phenomena of living beings, and prophesy that the time is soon coming "when the last vestige of the vital principle as an independent entity shall disappear from the terminology of science." [1]

Now, most of these gentlemen are not embarrassed by any very definite or detailed knowledge of the physiological and pathological phenomena which a tenable theory of life must be competent to explain, while they do know, or at least ought to know, a great deal of chemistry and physics; the confidence with which they maintain their creed is therefore readily understood. Much more surprising is it to find the same doctrine embraced by numerous zoologists, physiologists, nay, even pathologists, among them men who cannot for a moment be supposed to be unacquainted with the phenomena to be explained, and of whose abilities and reasoning powers it is impossible for me to think or speak otherwise than respectfully. Yet I cannot but believe that they have adopted the chemico-physical hypothesis, not so much because they are really satisfied with it as a scientific explanation of all the phenomena, as because they are unduly biased in its favor by the utterances of the great philosopher who has done, as I think we will all agree, such good service to biological science by elaborating and popularizing the doctrine of evolution.

It is only natural that such a bias should exist. The discussion of the nature of life—in the case of man at least—has always, and not unreasonably, been conjoined with the discussion of the nature of the soul, and the philosophers who have won highest repute in the latter discussion, have always been willing enough to offer solutions of the life-problem, and have never had any difficulty in finding followers even among those whose special lines of investigation might be supposed to impose upon them the duty of independent inquiry into the meaning of life.

Just as it was in the old time, with regard to this matter, so it is now. When Galen undertakes to discuss the complex phenomena of the Psyche, as manifested by the human species, he openly and continually confesses the extent to which he relies upon the authority of Plato; and when the dicta of the master are such as to require a special effort of faith on the part of the disciple, he honestly exclaims "Plato indeed appears to be persuaded of this, as for me, whether it be so or not, I am unable to dispute the question with him."[1]

In like manner, did they venture to be as frank as Galen was, most of the modern biologists who have adopted the chemico-physical theory of life would, I presume, confess "as to this matter our opinions are derived from Mr. Herbert Spencer's Principles of Biology—what are we that we should venture to dispute as to questions like these with him."

Nevertheless in striking contrast to this chemico-physical hypothesis of life, which is to be regarded as the fashionable faith of the hour, there still survives in many quarters, and especially among physicians, a disposition to regard indiscriminately almost all the phenomena of living beings as peculiar manifestations of a vital principle. So strong, indeed, is the faith of some of these modern vitalists, that they seem to shut their eyes to the evidence already in our possession as to the actual participation of known chemical and physical forces in the operations going on within living bodies, and appear almost to resent the willing aid that chemistry and physics afford to the physiological investigator of the present day.

Nay, further than this, in the inevitable reaction that is beginning to make itself felt against the avowed revival of the materialism of Epicurus and Lucretius—for we all know now that the chemico-physical hypothesis of life is not a new induction of modern science, but an ancient Greek speculation reappearing in modern petticoats—that other Greek speculation of the threefold Psyche, the doctrine taught by Plato and Aristotle, and which Galen accepted on their authority, the doctrine of a vegetable, an animal, and a rational soul, a human trinity coexisting in every human being, is once more rehabilitated and finding followers—likely, indeed, as I think, to obtain more followers than perhaps any of you yet suppose. And these followers are by no means confined to metaphysicians or churchmen, they can be found also already among the biologists. It is an English biologist of good repute, and of no mean abilities, who takes occasion, in a technical biological work published this very year, to express his belief that the Greek conception of the threefold Psyche "appears to be justified by the light of the science of our own day."[3]

For myself I must confess at once that I am quite unable to join either of these opposing camps as a partizan. I cannot accept the more strictly vitalistic views, because I am compelled continually to recognize the operation of purely chemical and physical forces in living beings. On the other hand, there are whole groups of phenomena characteristic of living beings, and peculiar to them, for which the chemico-physical hypothesis offers no intelligible explanation.

From this point of view the various processes and functions of living beings may indeed be divided into two classes, of which the first may be regarded with more or less certainty as the special results, under special conditions, of the very same forces that operate in the inorganic world; while the second, to which alone I would apply the term vital, are not merely in every respect peculiar to living beings, and hitherto utterly inexplicable by the laws of chemistry and physics, but are so different in character from the phenomena of the inorganic world that it does not seem rational to attempt to explain them by these laws.

Let me refer briefly to the processes and functions belonging to the first class. Here I place all those more strictly chemical processes by which, within the very substance of vegetable protoplasm, inorganic elements are combined into organic matter, as well as those which produce all the various subsequent transformations, whether in plants or animals, of the organic matter thus prepared. This general conception includes of course, in the case of the higher animals, all the chemical phases of the processes of digestion, assimilation and tissue-metamorphosis or metabolism, including secretion and excretion; in the case of the lower animals and plants, so much of these several functions as belongs to each species.

Now please to understand that when I say I recognize all the chemical phases of these processes to be the results of the ordinary chemical laws, I do not entertain any mental reservation with regard to the unrestricted application of these laws. I cannot for a moment agree with those physiologists who have imagined the vital principle to thwart, or interfere with, or counteract these laws in any way. I know, indeed, that we are far from being as thoroughly acquainted, as we may by and by hope to be, with the chemical phenomena of living beings; that many of the questions are very difficult, so that as yet, with all our labor, we have obtained but partial or even contradictory results; but I find in this only a reason for further investigation—no logical difficulty of a radical kind. In a general way I recognize that the matter of which living beings are composed is built up of elementary substances belonging to the inorganic world, and that it consists of atoms possessed of the very same properties, and obedient to the very same laws as like atoms in inorganic bodies. Yet I confess I find in all this no reason for denying the existence of a vital principle; only I do not figure this principle in my mind as a hostile power interfering in any way with the chemical tendencies of the atoms present; I liken its operations rather to those of the chemist in his laboratory who obtains the results he needs only on the condition of most rigid obedience to chemical laws.

Intimately associated with some of the chemical processes just enumerated are those chemical processes of respiration, in which the chemical affinities of the oxygen of the atmosphere are directly or indirectly the means of promoting tissue metamorphosis, as well as of reducing at once to simpler forms some portion of the various complex substances derived from the food. These chemical processes are undoubtedly the chief original sources of the heat and mechanical power manifested by animals. Of course they receive heat also from without by conduction and radiation; but this is a small matter to the heat generated within them; of course, too, mechanical power is continually transformed into heat within the body of animals, but this neither increases nor diminishes the total amount of energy liberated.

I yield my hearty assent to that modern scientific induction[4] which sees in the potential energy of the complex chemical compounds supplied to animals by their food, the essential source of all the actual energy of the body, whether manifested in the form of heat or work. In a general way the reduction of these complex chemical compounds by oxidation into the much simpler ones, urea, carbon dioxide, and water, is the means by which potential is converted into actual energy. In the case of plants, too, the source of any little heat that may be developed under special conditions, and of such sluggish motions as actually occur, is doubtless to be found in the reduction to simpler combinations by oxidation of a part of the organic matter already formed. The chief function of the vegetable world, however, is to build up, by means of the solar energy, those complex and unstable organic compounds that supply the animal world with food. Nevertheless, while I yield my hearty assent to this generalization, and freely admit that it is more than a mere deduction from the general doctrine of the conservation of energy—that in fact it affords the most satisfactory explanation yet suggested for a large number of observed phenomena—it is my duty to caution you against the erroneous supposition that any one has ever yet succeeded in affording a rigorous demonstration of the truth of the generalization by an adequate series of actual experiments.

Various attempts have, indeed, been made of late years to determine experimentally both for animals and for man, the potential energy contained in the food of a given period, and the actual energy liberated during the same time in the form of heat and work. I think, however, that all practical physiologists who have looked into the question will agree with me that the numerical results hitherto obtained must be received with the utmost caution.[5] Difficulties exist on both sides of the problem. It is comparatively easy, no doubt, to obtain a close approximation to the quantity and composition of the food; but to represent numerically what becomes of it in the body, to deduct correctly what passes through unchanged, and ascertain with reasonable accuracy the amount of carbon dioxide, water, and urea, into which the rest is transformed;

these are questions which have taxed the utmost resources of investigators, and as to which our knowledge is yet in its infancy.

On the other hand, the direct measurement of the resulting heat and work has hitherto proved still less satisfactory. It would seem to be a very simple thing to place an animal in a calorimeter, and measure the heat-units evolved in a given time, as Lavoisier and Laplace attempted to do in the latter part of the last century, and we have been told that "Lavoisier's guinea-pig placed in the calorimeter gave as accurate a return for the energy it had absorbed in its food as any thermic engine would have done."[6] But this assertion is not supported by the results of actual experiment. We know now that many precautions, unknown to Lavoisier, must be taken to secure any approach to accuracy in calorimetric experiments with animals, and just as the method is being brought to something like perfection by arranging for the respiratory process and its influence on the results, and by other necessary modifications of the primitive rude attempts,[7] doubts are beginning to arise as to whether after all the conditions in which the animal is placed in the calorimeter are not so far abnormal as seriously to vitiate the results;[8] so that in fact the most approved numerical expressions of the heat-production of the body to be found in the books are based rather upon calculation of the amount that ought to be produced by the oxidation of an estimated quantity of food than upon actual calorimetric observations.

Nor do we find it any easier when we attempt the actual measurement of the amount of work produced by an animal from a given amount of food. Indeed, in attempting to formulate an equation between the potential energy of the food and the actual amount of heat and work in any given case, we are met with the special difficulty that the animal does not evolve less heat because it is doing work than it does when it is at rest; on the contrary, it actually evolves more heat, consuming for the purpose more food than usual—or if this is not forthcoming, consuming a part of its own reserve of adipose tissue—so that from this source fresh complications of the problem arise.

The labor and ingenuity with which all these difficulties have been encountered is certainly worthy of the highest praise, and I willingly admit the probably approximate truth of the figures generally in use, say 2¼ to 2¾ million gramme-degrees as the daily average heat-production of an adult man, and 150,000 to 200,000 metre-killogrammes as his capacity for daily mechanical work.[9] Nevertheless these figures are after all only probable approximations, and there still exists, with regard to these questions, a large and inviting field for the application of chemical and physical methods to physiological research.

All the mechanical work done by living beings is effected by means of certain contractions of their soft tissues. The movements of the amœba, so often described of late years, may be taken as the type of the simplest form of these contractions. Similar movements occur, with more or less activity, in the protoplasm of all young cells, and in the higher animals are strikingly illustrated by the movements of the white corpuscles of the blood and the wandering cells of the connective tissue. In the lowest animal forms these simple amœboid movements of the protoplasm are the only movements, but in the higher forms, besides these, certain special contractile tissues make their appearance, by which the chief part of the mechanical work done is effected; these are the striated and unstriated muscular fibres.

On account of the extreme minuteness of the little protoplasmic bodies in which the amœboid movements are manifested, the investigation of the mechanical means by which these movements are effected has not as yet been attempted, although a great mass of details have been accumulated by actual observation with regard to the phenomena themselves and the conditions under which they occur. Very little more has been done with regard to the contractions of the unstriated muscular fibres. The striated muscles, however, have been made the subject of a host of researches, and I suppose the conclusions to which we may ultimately be led by these can be regarded, with but little reservation, as applicable to the function of the unstriated muscles, and also to the simpler amœboid protoplasmic contractions.

Yet, notwithstanding the vast amount of experimental labor and speculative ingenuity that has been lavished, since the time of Haller, upon the question of the contraction of the striated muscle, it must be confessed in the honest language of Hermann,[10] that the problem still mocks our best endeavors. For myself, I am unwilling to believe that the phenomena of muscular contraction, or indeed, of any of the varieties of protoplasmic contraction by which animals effect mechanical work, will not by and by be fully and satisfactorily explained on chemico-physical principles. I cannot for a moment give my adherence to the dogmatism of those modern vitalists who insist that the contractions of a muscle, or of an amœba, are essentially vital phenomena; for this would be to claim that life can create force. But it would be folly to shut our eyes to the circumstance that no chemico-physical explanation of muscular contraction yet offered has been so convincingly supported by facts as to command the universal assent of competent physiologists.

Of the various hypotheses devised to explain muscular contraction, those which regard the phenomena as in some way resulting from electrical disturbances have long enjoyed great popularity. Such of these hypotheses as still survive are based upon the electrical manifestations actually observed in living muscles. It has been pretty generally accepted in accordance with the observations of Du Bois-Reymond, whose brilliant series of experiments in animal electricity [11] is deservedly renowned, that even quiescent living muscles are in a state of electrical tension. If, for example, a muscle composed of parallel longitudinal fibres, be exposed with suitable precautions, and divided near each extremity by a transverse incision, the surface of the muscle will be found to be positive to the cut ends, and if one of a pair of non-polarizable electrodes, connected with a suitable galvanometer, is placed in contact with the surface of the muscle and the other in contact with one of the cut ends, the existence of a current is made manifest. The conditions are, moreover, such that while the maximum effect is produced when the equator of the surface is connected with the centre of one of the cut ends; more or less current will also be manifested whenever any two points of the surface are thus connected with the galvanometer, provided they are not equidistant from the equator. In such cases the point most distant from the equator is always negative. The electro-motive force of this natural current of the quiescent muscle varies greatly, but has been found by Du Bois-Reymond to amount sometimes to as much as .08 Daniell in one of the thigh muscles of the frog.[12] In muscles of different form, or cut differently from what has just been described, the currents are somewhat differently arranged, but the example just given must suffice for my present purpose.

In accordance with the observations of the same investigator, it is claimed that during a muscular contraction the electrical tension diminishes, the normal muscle-current experiences a negative variation, and this occurs in such a way, that as the wave of actual contraction moves along the muscle, which it does, according to the observations of Bernstein and Hermann,[13] with a velocity of about 3 metres per second, it is preceded by a wave of negative variation. This negative variation is indeed so trifling, if the muscle contracts but once, that it is difficult to observe it; but when the contractions succeed each other with great rapidity, as in artificially produced tetanus, it may become sufficient to neutralize completely the deflection of the galvanometer due to the current of the quiescent muscle.

But the belief that the electrical currents, shown to exist in the quiescent muscles in these experiments, exist also in uninjured animals has not remained unchallenged. Since 1867 it has been attacked especially by Hermann,[14] who has endeavored to show that these currents are produced only under the special conditions of the experiments, and that there are in reality no natural muscle-currents at all. It was well known that the currents observed in the experiments varied greatly under different circumstances, and it seemed a significant fact that they should be most intense when the muscle was removed from the body and had both ends cut off. If the muscle was removed with its tendinous extremities still attached, the current was usually found to be very feeble, or entirely absent, until the ends were well washed in salt and water, or dipped in acid. Du Bois-Reymond had explained this by supposing the natural ends of the muscle to be protected by what he called a parelectronomic layer of positive elements that must be removed before the natural current could be made manifest. On the other hand, Hermann has endeavored to show that the parts injured by the knife, or acted on by the salt or acid, enter at once into the well-known condition of rigor mortis, and only become negative to the still living portions of the muscle in consequence of this change. That electrical disturbances actually occur in contracting muscles he admits, but endeavors to show that they are due simply to the fact that the changes preceding contraction make the affected part of the muscle negative to every part less modified or wholly unaltered. Hence, if an uninjured muscle be caused, under proper precautions, to contract simultaneously in all its parts, it will be found that the contraction is wholly unaccompanied by any muscle-current.[15]

Observations that appear to support these views of Hermann have been brought forward by Englemann.[16] On the other hand

Du Bois-Reymond has defended his views with vigor, and sharply criticised, of course, the labors and logic of his assailant.[17] I need not at present express any opinion as to the merits of this voluminous controversy. It is enough for my purpose to indicate the questions at issue as sufficiently important and uncertain to be well worthy of independent experimental criticism.

Suppose, however, this criticism should result in showing that Hermann is wholly in the wrong, and that the muscle-currents observed by Du Bois-Reymond really exist in healthy muscles. How, then, shall these currents explain the phenomena of muscular contraction? I presume that no physiologist of the present day is misled by the superficial comparison, which Mayer and Amici were led by their microscopical studies of the muscles of insects to make between the striated muscular fibre and a Voltaic pile.[18] But the molecular theory by which Du Bois-Reymond has endeavored to explain his natural muscle-currents and their negative variation would appear to open up an inexhaustible mine of speculative possibilities for those who are inclined to speculate.

Yet the old experiment of Schwann[19] has always been a stumbling-block in the way of any theory that would explain muscular contraction by the action of a force which must increase inversely as the square of the distance between the molecules, for the force of the contraction, as it actually occurs, diminishes as the muscle shortens; and hence we find so good a physiologist as Radcliffe[20] reviving, in a modified form, the old hypothesis of Matteucci,[21] in accordance with which the electrical tension of the fibre, in the state of rest, causes a mutual repulsion of the molecules, and so elongates the muscle, while the contraction is merely the effect of the elasticity of the tissue, which asserts itself so soon as the repulsive force is diminished by the negative variation that precedes contraction.

In consequence of these and other difficulties many physiologists are beginning to regard the electrical phenomena as subordinate accidents of the chemical processes that go on in muscle, and endeavor to explain muscular contraction as resulting directly from these chemical processes themselves. Arthur Gamgee[22] has adopted as most probable the chemical hypothesis of Hermann.[23] This assumes the contraction to result from the decomposition of a complex nitrogenous compound supposed to be contained in the muscular tissue, and named inogen. During contraction inogen breaks down into carbon dioxide, lactic acid, (Fleischmilchsäure,) and gelatinous myosin. The rearrangement of molecules necessary to produce the latter body determines the contraction. Subsequently the gelatinous myosin combines with the necessary materials furnished by the blood, and becomes inôgen again. This decomposition and recomposition goes on also while the muscle is at rest, but, as then the gelatinous myosin is reconverted into inogen as rapidly as it is formed, no contraction results.

Du Bois-Reymond declares all this to be merely unsupported hypothesis.[24] Gamgee himself admits that it is, after all, not very clear why the gelatinous myosin should contract. Michael Foster,[25] who wholly rejects this particular chemical hypothesis, nevertheless seems quite sure that the true explanation will be found to be a chemical one. He insists that muscular contraction is essentially a translocation of molecules, and declares that whatever the exact way in which this translocation is effected may be, it is fundamentally the result of a chemical change, or, as he describes it, "an explosive decomposition of certain parts of the muscle-substance."

The purpose I have in view does not require, fortunately, that I suould attempt to decide whether these more purely chemical theories of muscular contraction, or the more purely electrical theories, are best entitled to confidence. My object has been effected, if I have impressed you with the fact that wide differences of opinion still exist as to the nature of the process, and that further investigation is indispensable for the settlement of existing controversies.

The subject just briefly discussed brings us naturally to the consideration of the nature of the action of the motor nerves, by which, in all animals possessed of a muscular and nervous system, the contraction of the muscles is regulated and determined.

The hypothesis which identifies the nervous currents with electricity was propounded in the posthumous work of Hausen[26] in 1743, and, notwithstanding all the difficulties and objections it has encountered, still survives in a modified form in many contemporaneous minds. Those who hold to this view appeal in its support to the electrical phenomena actually observed in nerves in accordance with the investigations of Du Bois-Reymond. These observations have long been widely accepted as conclusive proof that natural currents exist in the quiescent nerve of the same general character as those attributed to the quiescent muscle, which I outlined a few minutes ago. The electro-motive force of this current was found by Du Bois-Reymond[27] to be equal to .022 Daniell in the sciatic nerve of the frog. When a nervous impulse passes along the nerve the natural current is diminished; it experiences a negative variation, which, according to Bernstein,[28] when the impulse results from a very potent stimulation, may more than neutralize the natural current. The same physiologist has shown that this negative variation moves along the nerves of the frog at the rate of 28 metres per second; that is, at the same rate as the nervous impulse itself, as determined without reference to the electrical phenomena.

As in the case of the muscle-currents, these phenomena have been differently interpreted by Hermann,[29] who denies the existence of any natural nerve-current in uninjured nerves, and ascribes those observed in the experiments to the circumstance that the parts of the nerve dead or dying, in consequence of the section, become negative to the living nerve. The negative variation produced by the stimulation of a nerve he explains by assuming that the stimulated part of the nerve becomes, in consequence of the changes resulting from the stimulation, negative to the unstimulated parts. I will not attempt to enter to-night into the merits of the controversy still in progress with regard to this question; nor will I pause to discuss the exceedingly curious and interesting phenomena of electrotonus,[30] concerning which, I will only say that the question has even been raised by Radcliffe as to how far these phenomena are peculiar to nerves, and how far they may be regarded as mere phenomena of the electrical currents employed, which would be equally manifested under similar circumstances if a wet string or other bad conductor should be substituted for the nerve.[31]

However these disputes may be ultimately decided; whatever the actual facts with regard to the electrical manifestations in nerves at rest or in action, may ultimately prove to be, there is a group of easily repeated elementary experiments which seem to show pretty distinctly that whatever the nervous impulse may be, it is not merely an electrical current.

It was known already when Haller wrote[32] that a string tied tightly around a nerve, although it in no wise interferes with the passage of electrical currents, puts a speedy end to the transmission of nervous impulses. With this old experimental difficulty uncontradicted, it seems strange that anyone should declare at the present time that "the main objections raised to the electrical character of nerve energy is based upon its slow propagation."[33] In fact this latter objection is altogether a subordinate difficulty which may perhaps be entirely explained away; the main experimental objection does not relate to the velocity, but to the conditions of the propagation of the nervous impulse. If, instead of tying a string around it, the nerve be merely pinched or bruised well with a pair of forceps so as to destroy its delicate organic texture; if it be compressed tightly by a tiny metallic clamp; if it be divided by a sharp knife, and the cut ends brought nicely into contact, or brought in contact with the extremities of a piece of copper wire, it will still conduct electrical currents as well as ever, but can no longer transmit the nervous impulse. So, too, there are certain poisons, such as the woorara, which completely destroy the capacity of the nerve for transmitting nervous impulses, without in the least diminishing its conductivity for electricity.[34]

In view of these and other practical difficulties, the best instructed modern physiologists no longer attempt to identify the nervous impulse with the electrical phenomena by which it is accompanied. Du Bois-Reymond himself has suggested that the nervous agent "in all probability is some internal motion, perhaps even some chemical change, of the substance itself contained in the nerve-tubes, spreading along the tubes."[35] Herbert Spencer came to the conclusion that "nervous stimulations and discharges consist of waves of molecular change"[36] flowing through the nerve-fibres; and I suppose that most physiologists at the present time think of the nervous current in some such way as this. Even those who attach most importance to the electrical phenomena will, I take it, agree with Michael Foster, that these "are in reality tokens of molecular changes in the tissue much more complex than those necessary for the propagation of a mere electrical current."[37]

We do not, however, as yet possess any sufficient foundation of facts on which to build a reasonable hypothesis as to the nature of the molecular disturbances that accompany a nervous impulse. The labors of the physiological chemists have taught us nothing with regard to the changes that go on, except that the axis-cylinder which, in the inactive living nerve is alkaline, becomes acid after long continued activity, or after death.[38] We can measure the velocity with which the impulse travels; we can study the conditions under which it arises; we can believe, as I certainly do, that it will ultimately receive a chemico-physical explanation, but its real nature we do not yet know.

So far as we can ascertain, the phenomena of the conduction of nervous impulses by the sensitive nerves are so similar to those of the conduction of motor impulses, that any explanation ultimately adopted for the one will probably apply to the other also. When, however, we ascend to the study of the nervous centres, by which sensitive and motor nerves are connected together, and attempt the interpretation of the complex functions of nerve-cell, ganglion, spinal cord, and brain, we find that none of the hypotheses hitherto brought forward to explain the observed phenomena repose on any defensible chemico-physical basis.

I cannot, of course, undertake to give to-night even the most meagre outline of the wondrous mechanism which physiological experiments show must exist. That reflex actions, co-ordinated muscular movements, and all the complex phenomena of this class, do depend upon a wonderfully complex mechanism, and occur in strict accordance with the ordinary chemical and physical laws, I do not for a moment doubt, and I cordially invite the co-operation of the chemists and physicists to aid the physiologists in the explanation of this mechanism, for we stand only upon the threshold as yet.

If now we turn from the more general discussion of muscular contraction and nervous action, to the consideration of the several functions carried on in animals, by means of special arrangements of the muscular and nervous systems, we continually encounter the preponderating influence of purely physical laws. The introduction of air into the lungs of breathing animals, and its expulsion thence, is effected in a purely mechanical way, while the exchange of the carbon dioxide of the blood with the oxygen of the inspired air occurs in strict obedience to the laws of the diffusion of gases.

The ordinary laws of hydraulics govern the circulation of the blood and lymph, and all the complex visible motions of the body are executed in accordance with the ordinary laws of mechanics; nor is it at all necessary for me to insist upon the purely physical nature of the operations of the organs of the special senses, conspicuously the eye and the ear. For example, so far as concerns the means by which images of external objects are formed sharply upon the retina, the eye is as purely a physical instrument as the telescope or the microscope. But I need not dwell upon this group of phenomena, because the importance of the role of the ordinary physical laws in this domain is conceded, I suppose, by the extremest of the vitalists of the present day.

We see, therefore, that, with regard to a large part of the phenomena of living beings, there are grounds for affirming either that they have already been satisfactorily explained by a reference to established chemical and physical laws, or at least that they are of such a character that it is reasonable to hope they may be thus explained at some future time. Is it possible, then, to return, as some have done of late years, to the old speculation of Des Cartes, and look upon living beings as mere machines? To do so, it will not suffice to image to yourselves ordinary machines in which fuel yields force. To satisfy the chemico-physical hypothesis of life you must suppose machines that build themselves, repair themselves, and direct, from time to time, new applications of their energy in accordance with changes in the environment; nay, more—machines that accouple themselves together, breeding little machines of the same kind that grow by and by to resemble their parents, and all this self-directed, without any engineer. But even Des Cartes required an engineer—the soul—to run his man-machine, and the logic which compelled him to this view applies just as forcibly to all the modern machine conceptions of living beings.

I have already asserted that there are whole groups of phenomena characteristic of living beings, and peculiar to them, which cannot be intelligently explained as the mere resultants of the operation of the chemical and physical forces of the universe. These phenomena I refer—I avow it without hesitation—to the operations of a vital principle, in the existence of which I believe as firmly as I believe in the existence of force, although I do not know its nature any more than I know the nature of force. If, for convenience, at any time, I compare the living body to a machine, I must compare the vital principle to the engineer—it is the director, the manager if you will, but it does not supply the force that does any part of the work. Let us consider, then, in the remainder of this discourse, the phenomena which indicate the guidance of the vital principle.

The first group of phenomena belonging to this second class are those forced upon our attention whenever we attempt to study the question of the origin of life. It has seemed to some of our contemporaries that, in accordance with the doctrine of evolution, as deduced by Mr. Herbert Spencer from the great truth of the persistence of force, life ought always to arise spontaneously out of inorganic matter whenever the necessary materials and other conditions of life are brought together. Indeed, if there be nothing more or other in life than force, I confess I do not understand how this conclusion can be logically escaped; and yet, when we come to interrogate nature, we find that, in point of fact, things do not happen so.

The sun may stream all the enormous energy of his rays upon the slime of the Nile, but he generates no monsters; nay, not even a bacterium, except in the presence and under the direction of pre-existing life. Our biological knowledge has so far advanced that it is easy for us to get together mixtures of matter, for the most part derived from pre-existing living beings, which are peculiarly well fitted to supply the materials needed for the building up of a variety of low forms of life, and the extent of our present knowledge of the conditions favorable to the development of these low forms of life is shown by the rapidity with which they do develop from a few individuals to countless millions, if only a few individuals are introduced as parents into our flasks and brood-ovens. The species to which the countless progeny belongs, depends always upon the species of the parents we introduced by design or accident, and if parents of several species are introduced we may imitate on a tiny scale the great struggle for existence, and witness the survival of the fittest. Never, however, has the spontaneous generation, out of inorganic matter, of a single living form been yet observed.

Speculative considerations have, indeed, from time to time led certain enthusiasts to desire earnestly that it might be observed; and when we consider on the one hand the influence of pre-existing bias, and on the other the intricacy of some of the experimental processes in question, it is by no means necessary to charge dishonesty upon those who, from time to time, have actually fancied that their desires have been realized to the extent of the spontaneous generation of bacteria at least. When we consider the immense development of the trade in canned food, which could not exist for a single summer's day, if these experimenters were not mistaken, it will be seen how little need there was for renewed scientific experiment to refute their conclusions; but it is a noteworthy fact that among those who have contributed most by exact research to recent scientific demonstrations of the truth, that life never arises except from pre-existing life, are to be found some of the most earnest and eloquent advocates not merely of the doctrines of evolution, but of its supposed corollary, the chemico-physical hypothesis of life.

I sympathize heartily with those who, recognizing that the supposition of the spontaneous origin of life on our globe is flatly contradicted by the facts of science, have endeavored to escape the difficulty by imagining the earliest parent living forms to have been brought to our earth on the surface of meteoric stones or other cosmical bodies. This hypothesis, put forward originally on purely theoretical grounds, has recently acquired a certain degree of support from the published observations of Hahn and Weinland,[30] who believe they have recognized the remains of humble coralline forms in thin sections of meteoric stones collected in Hungary. Yet these observations, if indeed they should prove to be correct, would rather afford indications of the existence of life in other worlds than ours, than show that living forms could survive the high temperature to which such cosmical masses must be exposed during their transit through our atmosphere; and even should we find reasons for ultimately adopting this hypothesis, we should not have solved the problem of the origin of life, but only removed it entirely beyond the domain of further scientific investigation.

If, however, we reject this view, and still mean to support the chemico-physical hypothesis of life, we shall have to resort to a still more improbable supposition. We shall have to suppose that although in the present order of things life can only arise out of pre-existing life, the order of things was at some past time so far different that life could then arise out of inorganic matter; a supposition which implies an instability in the course of nature that is contradicted by all the teachings of science.

I willingly admit that, in view of our present scientific notions of the cosmogony, it is impossible to believe that life always existed upon this planet. I willingly admit that life on the earth must have had a beginning in time. But we do not know how it began. Let us honestly confess our ignorance. I declare to you I think the old Hebrew belief, that life began by a creative act of the Universal Mind, has quite as good claims to be regarded a scientific hypothesis as the speculation that inorganic matter ever became living by virtue of its own forces merely.

If we turn now to the consideration of the processes of growth, we shall find additional reasons for believing in the existence of a vital principle. Let us consider first, in the most general way, the conditions under which those strictly chemical processes occur, to which I have already alluded, and by which the inorganic atoms are combined into organic matter. I repeat it, I do not for a moment question that the actual force by which these processes are compelled exists in the solar rays, and that it is, after all, the solar energy thus stored up in the vegetable protoplasm and its products that supplies, by its subsequent liberation, all the force manifested by living beings. Yet, let me beg you to observe that in all the myriads of years during which the solar energy has streamed upon the earth, that energy has never, on any occasion that we know of, determined the combination of inorganic atoms into organic matter, except within the substance of already living protoplasm. The water and carbon dioxide and ammonia in the atmosphere and in the soil, come into contact with each other, within the substance of porous inorganic clods on the surface of the soil, much as they do in the substance of protoplasm, and the equal sun warms both alike; but in the clod they remain water, carbon dioxide, and ammonia; in the protoplasm, provided only that it is living protoplasm, they combine into starch or oil, or even into protoplasm itself. The essential condition, then, of this storing up of the solar energy for the subsequent use of living beings is the presence of life, and in these fundamental operations the mighty force of the sun acts, in the fullest sense of the words, the part of the servant of life.

The view thus suggested, that we have here to do with something more than the mere operation of the inorganic forces, is still further strengthened when we come to consider more in detail the phenomena of the growth of living beings, whether plants or animals. The better we become acquainted with these phenomena the more fully we become convinced that we have to do with processes for which the inorganic world affords no parallel.

Linnæus, indeed, declared, "lapides crescunt," using the very same phrase which he applied also to plants and animals.[40] But it is impossible to maintain this assertion without adopting the most superficial view of the growth of living beings, and defining the process to consist merely in increase of size. That this should have appeared reasonable, in the time of Linnæus, need excite no surprise; but it seems strange to find so astute a thinker as Mr. Herbert Spencer repeating the old fallacy in the first chapter of his Inductions of Biology, and declaring: "Crystals grow, and often far more rapidly than living bodies."[41] Then, after instancing the formation of geological strata by the deposit of detritus from water, as well as the formation of crystals in solutions, as examples of growth in the inorganic world, he asks: "Is not the growth of an organism a substantially similar process?" and adds: "Around a plant there exist certain elements that are like the elements which form its substance, and its increase in size is effected by continually integrating these surrounding-like elements with itself; nor does the animal fundamentally differ in this respect from the plant or the crystal."

Now, as opposed to this, I must express my belief that the more we know of the actual details of the process of growth in plants and animals the more clearly it will be seen that this process does differ so fundamentally from that by which a crystal is formed and increases in size, or from any increase in size of inorganic bodies, that the same scientific term cannot, with any propriety, be applied to both, however long popular usage may have given to both a common name. When inorganic bodies increase in size the additional atoms are deposited on their external surfaces; or, if a fluid, after penetrating the interstices of some porous body, deposits there any material held in solution, the mass, indeed, is increased thereby, but not the size. When, however, vegetable protoplasm grows, it does not merely integrate with itself certain elements around it like the elements which form its substance; the needed elements exist in compounds quite unlike itself, and it combines them together into protoplasm in all parts of its mass, so that it grows by a process of intussusception wholly unlike anything that occurs in the inorganic world. In the case of animal protoplasm, the mode of growth by intussusception is the same, but the capability of combining together mere inorganic elements into its own substance is lost; and, besides these, a certain amount of pre-existing vegetable or animal protoplasm must be present in the food, or growth will not go on.

In both cases, when the growth has proceeded to a certain extent—within certain definite limits—a new characteristic phenomenon occurs in a growing mass of vegetable or animal protoplasm; it multiplies by division, its whole mass participating in the act, in accordance with one or other of a few definite methods. This process is repeated again and again. The progeny may separate, without modification, as independent forms, or, as in the case of the more complex organisms, they may cohere together, and the process culminates by groups of them undergoing certain definite and peculiar transformations, after which further multiplication becomes rare or ceases altogether, and the growth of the complex organism is thus limited.

I cannot, of course, attempt this evening to describe all the known details of the process of growth which I have thus hastily sketched; to give you a really satisfactory account of them would require a series of lectures. But I do not hesitate to say that the more fully you know these details the more unscientific you will think the attempt to class them as in any way similar to the circumstance that inorganic crystalline compounds seem "each to have a size that is not usually exceeded without a tendency arising to form new crystals, rather than to increase the old." It is, at the best, a waste of words to attempt to explain complex phenomena by comparing them to simpler ones which are fundamentally unlike them.

I have but now referred to a process by which, in the growth of the more complex living beings, the small primitive protoplasmic mass, out of which each individual arises, subdivides and produces a numerous brood of protoplasmic masses, at first closely resembling the parent mass, but after a time differing from it more and more, and finally undergoing transformations into definite and peculiar forms. This process, which does not take place in any disorderly manner, but in a very characteristic and definite way in each individual form, is designated by the term development. In point of fact, so far as it consists in the mere growth and multiplication of the individual elements that compose the organism, and the increase in size of the organism itself on account of these processes, it is properly designated by the term growth. In so far, however, as the individual elements are differentiated, and the wonderful architecture of the living being, with its organs and systems, is completed thereby, it is properly designated by the term development.

Nothing like the process of development as thus defined exists in the inorganic world, and in all the attempts at such a comparison that it has been my fortune to meet, the most fundamental facts of the development of living beings have been persistently ignored. Among these fundamental facts I invite your attention especially to the circumstance that there is something in the miscroscopic mass of protoplasm, out of which, even in the case of the highest and most complex living beings, each individual arises, that goes even further in determining the direction in which the individual shall develop than the pabulum, or environment, or all the mighty chemical and physical forces that are brought into play as the process goes on. In a word, the individual developes after the pattern of its parent, or not even all the solar energy can compel it to develop it at all.

We are thus brought face to face with the facts of sexual generation, and especially of heredity, with all their wide bearings on the great biological questions of natural selection and the origin of species. Into the details of these large questions the limits of the hour will not permit me to enter. Could I take time to do so, I am satisfied that at every step I should be able to collect for you additional evidence of the existence of a vital principle. Still I regret this the less because most of you, I think, are so familiar with the modern literature of these subjects, and especially with the admirable writings of Mr. Darwin, that I feel sure, if I can succeed in giving you a clear outline of my views, much that I should say, had I time, will suggest itself to your own minds. In a general way, however, when we study, in the history of life upon this globe, the double phenomena of long continued persistence of type, and of slow variation continually occurring, we will find that almost all biologists, whatever their theory of life, explain these phenomena on the one hand by heredity, on the other by the sensibility of the organism to the influence of the environment.

Both heredity and the influence of the environment may be very conveniently studied in those simplest organisms in which each individual consists of a single minute mass of naked protoplasm, as in certain rhizopods, for example, the amœba. These tiny creatures produce a progeny which preserves the parental type as closely as is done by the offspring of the higher animals. Their sensibility to the influence of the environment is manifested in several ways. They grow, that is they appropriate materials from the environment, in the way I have already specified; they manifest automatic movements, that is, on encountering food, obstacles, or other disturbing external circumstances, movements result the direction and energy of which are in no wise determined by the character or force of the external influences, or as they may be conveniently termed the stimuli by which these movements are provoked; and finally, simultaneously with the process of growth, a certain metamorphosis, or metabolism, of the protoplasm is continually going on resulting in the formation of excrementitious substances which are continually being excreted.

The processes of growth and metabolism exhibit different degrees of intensity in accordance with variations of the environment, and whatever physical theory of the mode in which the protoplasmic motions are produced we may adopt, the mechanical force manifested can only be supposed to proceed from the decomposition of a part of the protoplasm itself into simpler compounds, that is, from a particular kind of metabolism. Hence you will I think, be quite prepared to hear me speak of all the circumstances in the environment that so act upon living protoplasm as to increase its growth or metabolism, as stimuli, and of the property of living protoplasm by which all its responses to stimuli are guided, as irritability, instead of limiting these terms to the phenomena of automatic movement only, as was formerly done. This irritability of living protoplasm determines the direction in which its internal forces shall be manifested. Speaking of it as I do, perhaps you would wish me to call it sensibility rather than irritability, and I do not know that I should object very strenuously to any one who wished to do this. But however you may name it, it is this vital property of all living protoplasm that produces the sensibility to changes in the environment which has been the main factor in the gradual evolution, during the ages, of the highest and most complex from the simplest and lowest living forms.

Against this view it has been urged with much ingenuity that protoplasm is the material substratum of life, and life merely a property of protoplasm; that is, if the words have any meaning at all, that life is the resultant only of the forces inherent in the inorganic atoms of which the protoplasm is built up. Now, in the first place, no one has ever yet been able to show, by any conceivable synthesis, how the forces known to belong to the several kinds of inorganic atoms of which protoplasm is composed, could by their combination, produce the characteristic phenomena of living protoplasm, namely, the phenomena of irritability, as I have just described them. But, in the second place, this speculation appears to be pretty flatly contradicted by the circumstance that, although protoplasm can only be formed within the substance of previously existing living protoplasm, it can continue to exist, it does continue to exist as protoplasm after it has ceased to live. Not merely can it persist for a time without chemical change as dead protoplasm, it can subsequently serve as food and be reconverted into living protoplasm once more. Bear in mind, however, that this change can only be effected within the substance of the living protoplasm of the animal that assimilates this food. It is not effected by the chemistry of digestion, that merely makes peptone of the protoplasm; merely makes it soluble enough to pass into the substance of the protoplasmic masses that are to appropriate it. These considerations, then, would seem to show that the material, protoplasm, cannot be rightly believed to be of itself the cause and essence of life.

If I should pause here, it seems to me that I should have brought forward adequate reasons for believing in the existence of a vital principle. But I cannot pause here. Beyond and above all this there is another great group of phenomena peculiar to living beings— a group of phenomena concerning which, in my own individuality, I have knowledge at least as positive as any I possess of the existence of force, and which I am led, by a logic quite as convincing as that by which any general proposition with regard to the external world is proven, to believe exists in like kind and degree in the case of my fellow-man. I refer to the phenomena of the perceiving, emotional, willful, reasoning human mind. Into the argument that makes it highly probable that a similar but less and less perfect mind exists in the animal world, and identifies with mind the sensibility of the lowest animal forms, and even that of vegetable protoplasm, I will not attempt to enter to-night. Mr. Herbert Spencer himself has presented this view with so much ingenuity, that, without committing myself to an approval of all his details, I must content myself by referring you to his writings for one of the best discussions of this matter. It will be sufficient for my present purpose to close this discourse by the presentation of a few considerations in relation to mind as it exists in man.

For myself I know mind only as a manifestation of life, if indeed it is not the essence of life. But the old doctrine of Epicurus, handed down to us in the poem of Lucretius, that in some way or fashion mind is produced by the clashing together of the atoms, has been boldly revived of late years, and transmuted into a form more plausible to modern thought, although just as unsupported by any actual knowledge of facts.

No one has done this more boldly or more cleverly than Mr. Herbert Spencer has done in his First Principles, and of course you are all familiar with the ingenious argument, in favor of this view, which runs through that masterly work. It would be, from many points of view, profitable, but it would be a very laborious task to attempt the critical discussion of his argument. It must suffice, for my present purpose, to point out that two of the fundamental assumptions upon which that argument is based are wholly undemonstrated. The first assumption is, that mind is itself a force;[42] the second, that mind cannot be conscious of itself, but only of the external world.[43]

If I could bring myself to believe that mind is, in any proper sense of the word, a force, and that such popular metaphorical expressions as mental force or mental energy accurately described the phenomena, I should certainly expect to find at least some shadow of proof for Mr. Herbert Spencer's assertion, that mental operations fall within the great generalization of the correlation and equivalence of the forces. On the contrary, however, you will find, on reading his lucid periods, that his whole argument relates to those physical conditions in the organs of sense and in the muscular and nervous systems, which are the antecedents of perception—which are, in fact, the things really perceived—and in no sense constitute the perceiving mind. Between strictly mental phenomena and the physical forces no one has as yet even attempted to establish a numerical equivalent; nay, more, the correlation of thought with the physical forces is not only undemonstrated, it is utterly unthinkable. You can conceive several different ways, it matters not whether true or false, in which the motions we know as heat might be converted into those we know as light, and so on with the other physical forces; but you cannot represent mentally any intelligible scheme by which any of the physical forces can be converted into the simplest or most elementary thought.

As to the question of self-consciousness, it seems as if the great philosopher were reasoning in a circle. He first assumes that the fundamental condition of all consciousness is the antithesis between subject and object,—which is true only with regard to consciousness of perception, the form of consciousness by which we become acquainted with the non ego,—and then he concludes that there can be no consciousness of the ego because it cannot fulfil these conditions. That is, in a word, he denies consciousness of the ego, because it is not consciousness of the non ego. Really it appears to me that, as against such a philosophy as this it is not amiss to appeal to "the unsophisticated sense of mankind," of which Mr. Mansel speaks.[44] But there is fortunately a better philosophy than this; a philosophy which recognizes the validity of the mind's self-consciousness as at least fully equal to the validity of its consciousness of the conditions of the body by which it obtains a knowledge of the external world. By this self-consciousness I know, with a certainty which no doubt can ever disturb, that I have a mind; and by rightly applying my reasoning powers to the data of my self-consciousness, I can learn much that will be useful to me with regard to my mental processes and the methods of employing them. But here I have to stop. I can learn nothing, whether by consciousness or by reasoning, with regard to the real nature of my conscious mind, and however much it may long for immortality, neither philosophy nor science afford any foundation of proof upon which it might build its hopes.

I have already said that I know mind only as a manifestation of life. Its operations are intimately connected with the chemical and physical phenomena of living beings, and it exercises over them a certain directing influence, the nature of which we do not understand. The obedience of our voluntary muscular actions to the mandates of the guiding will is a familiar illustration of this directing influence. On the other hand, all the knowledge of the external world on which the mind exerts its reasoning power reaches it through the organs of sense and the nervous system. Indeed, our studies of the phenomena of sensation compel us to conclude that what our mind really perceives, when it takes cognizance of the external world, is merely the ever-changing panorama of our own cerebral states. It should be anticipated, therefore, that disturbed or morbid conditions of the brain would lead to irregular or disorderly mental operations; and the circumstance that this really happens, affords no better proof of the materiality of thought than is afforded by the circumstances of our ordinary normal thought.

So, too, since the cerebral changes, which the mind perceives, are themselves of a purely chemico-physical nature, it should be anticipated that, like the metabolic processes in other tissues, they would be accompanied by an increased excretion of characteristic waste-products, by evolution of heat and by afflux of blood. Experimental investigation has been directed to each of these points, and some important observations have no doubt been made; but much of the testimony is conflicting, and our knowledge is still so incomplete that further inquiry in each direction is greatly to be desired.

This is particularly the case with regard to the chemical questions connected with the metabolism of the brain. In the first place our knowledge of the chemical composition of brain-substance is still in its infancy. The view that its characteristic ingredient is the phosphorized nitrogenous body described in 1865 by Liebreich under the name of protagon has been strongly controverted by Diaconow, Hoppe-Seyler, and Thudicum, while recently it has been reaffirmed by Gamgee, and Blankenhorn.[45] But even should this view turn out to be well founded, we have yet everything to learn with regard to the transformations protagon undergoes during functional activity, and the nature of the resulting waste products.

Long before Liebreich announced the existence of protagon, however, the attention of the physiological chemists had been directed to the prominence of phosphorous as an element in the composition of the cerebral substance, and it had been suggested that a part of the phosphoric acid excreted in the urine might be derived from the metabolism of the brain. As early as 1846 Bence Jones[46] had observed an excess of phosphatic salts in the urine during certain brain diseases, notably acute inflammations, and an observation published in 1853 by Mosler[47] appeared to indicate that a similar excess followed intellectual activity.

Byasson [1868] in his essay on the relation between cerebral activity and the composition of the urine,[48] reports a number of urinary analyses which support the view that the excretion of alkaline phosphates by the kidneys is habitually increased during mental work. This opinion has also received a certain degree of support from the more recent papers of Zuelzer[49] and Struebling;[50] nevertheless it is impossible to study the detailed observations upon which it is based without feeling how meagre and unsatisfactory the evidence relied upon really is. It is at best only sufficient to indicate the importance of further inquiry, and to suggest the necessity of avoiding certain obvious errors of method which complicate and obscure the results of the investigations hitherto made.

The opinion that mental effort is accompanied by an increase in the temperature of the brain was first propounded by Lombard in 1867. Using a delicate thermo-electric apparatus of his own contrivance, he observed during mental effort a rise of the surface temperature of the head, which sometimes amounted to as much as one-twentieth of a degree centigrade.[51] Subsequent and more elaborate investigations confirmed him in this conclusion, which has also been supported by observations made with thermo-piles by Schiff and Bert, as well as by the use of surface thermometers in the hands of Broca and L. C. Gray of Brooklyn.[52] Gray claimed to have observed a maximum rise of as much as two and a half degrees Fahrenheit. These physicians and some others have also investigated the relative temperature of the two sides of the head, of different regions on each side, the variations produced in certain regions by voluntary muscular movements, and those resulting from localized brain diseases.[53]

To attempt any discussion of these interesting studies, and their conflicting results, would lead me altogether beyond my prescribed limits. It is enough for my present purpose to point out that the recent investigations of François Frank[54] would seem to indicate that the variations of temperature actually observed are chiefly due to changes in the cerebral circulation. Plunging suitable sounds, connected with a thermo-electric apparatus, into the brains of animals to different depths, Frank found that the deeper parts of the brain are always warmer than its superficial layers. The superficial layers are continually cooled by radiation, and their temperature is a degree, or more than a degree centigrade, lower than that of the deeper parts. Even these, however, are .1° to .2° centigrade cooler than the blood in the thoracic aorta, and it will therefore readily be understood that a relaxation in the muscular coats of the cerebral vessels, permitting the more rapid circulation of a larger quantity of blood, would be promptly followed by an increase in the temperature of the superficial parts of the brain. None of the observers I have cited have reported a surface temperature of the head during mental effort that is too high to be accounted for in this way; and if, as I willingly concede is probable, there is really an increased heat-production in the brain itself, it is wholly masked by the more considerable change due to afflux of blood.

Now a consideration of the phenomena of blushing, and certain well known sensations in the head, might lead us to expect that emotional and mental conditions would prove to be attended by increased activity in the circulation of the blood in the brain; yet many difficulties have hitherto been encountered in the attempt to demonstrate experimentally that this is true. Mosso of Turin supposed that he had succeeded in doing this with his plethysmograph.[56] The instrument is essentially a cylinder of water, into which the arm is introduced and so fastened in place by a caoutchouc membrane that the slightest increase or diminution in the volume of the arm will cause the rise or fall of the water, through a tube connected at one end with the interior of the cylinder and at the other with a suitable recording apparatus. The pen or pencil of this apparatus inscribes a curve that rises or falls with the fluid in the tube. Among the curious observations made with this instrument, Mosso reports that the mental operations and emotions of the persons he experimented on were accompanied by a fall of the curve, which he regarded as proof that more blood goes to the brain and less to the arm during emotion, or mental action, than at other times. But the following year these observations were repeated with great care, and with an improved plethysmograph by Basch, of Vienna,[56] who failed to verify them. Most of the phlegmatic Germans on whom he experimented did sums in their heads, and otherwise exerted their minds, without producing the slightest modification of the curve, and none of them appear to have been as emotional as Dr. Pagliani, of whom Mosso relates that, his arm being in the plethysmograph, when the revered Prof. Ludwig entered the room the curve fell as if he had received an electric shock. Basch has cautiously investigated the causes of the varying quantity of blood in the arm in these experiments, and has clearly shown how many general and local conditions concur in producing the result. Especially has he emphasized the effect of variations in the abdominal circulation, which appear to exercise a much more considerable influence upon the size of the arm than any changes that occur in the brain.

In subsequent works Mosso has stated that during mental effort, such, for example, as is required to multiply small numbers in the head, the radial pulse, as recorded by the sphygmograph, is shown to become somewhat more frequent, and the recording lever does not rise so high as at other times.[57] Thanhoffer, who has pointed out that in these observations the influence of respiration on the pulse was neglected, concluded, nevertheless, from his own sphygmographic observations, that after due allowance is made for this complicating influence, it must be conceded that cerebral activity does exercise a certain effect upon the pulse, and in the direction stated.[58] Eugène Gley, in a recently published essay, claims to have obtained similar results, and states that at the same time the sphygmographic trace of the carotid artery shows a higher upstroke of the recording lever, and other indications of dilatation of the vessel.[59] While these observations are not sufficiently numerous, or free from objections, to be accepted without question as proof that an increased supply of blood to the brain invariably accompanies mental effort, they are certainly sufficient to encourage further labor in this interesting field.

But if the arguments in favor of the purely material nature of our mental operations that have been based upon the imperfect results of the three lines of investigation I have just referred to must be rejected as utterly fallacious, what shall we say of the logic that attempts to draw a similar conclusion from the results of those inquiries into the phenomena of personal equation which aim at determining the time that must be allowed for the mental operation involved?[60] Do we, then, indeed need the beautiful experiments of Hirsch and Donders[61] to prove that thought occupies time? Whence, indeed, do we derive our primitive conceptions of time save from our consciousness of the succession of thought? And how could even the shortest time be occupied by even an infinite number of thoughts if each thought did not occupy at least some time, however brief?

I have thus, gentlemen, attempted to show that we are logically compelled to invoke the existence of a vital principle in order to account for certain important groups of phenomena occurring in living beings which cannot possibly be explained by the chemical and physical forces of the universe. These phenomena form a series, at one end of which we find the mere irritability or sensibility of the humblest mass of living protoplasm; at the other the reasoning faculty of the human mind. From the one extreme of this series to the other I recognize the manifestations of the vital principle. I willingly confess that I know nothing of the ultimate nature of this principle, except that it must be very different from the chemical and physical forces whose operations I have learned to recognize in the organic as well as in the inorganic world; nevertheless I am compelled by my study of the phenomena to conclude that it exists. I know that Mr. Huxley, only last summer, declared in the International Medical Congress at London, that the doctrine of a vital principle is the "asylum ignorantiæ of physiologists;"[62]

but this ancient sarcasm has now been applied to so many things that it has long since lost whatever sting it may once have possessed, when it was fresh and new. And I also know that one of the chief characteristics of true science is the sharpness with which it enables us to discriminate between that which we have proven and really know and that which we have not proven and do not know. Better far is it, and a thousand times more in accord with the simple honesty of science, to acknowledge frankly the truth that phenomena occur in living beings which the inorganic forces do not explain, than to mistake our wishes for discoveries, to convert conjectures into dogmas, or, worst of all, to transform an undemonstrated hypothesis into a superstitious, aggressive, and intolerant creed.

Nor will the soundness of the conclusions, at which the present generation shall arrive as to this matter, be without its practical effect upon methods of biological research, and the consequent future progress of biological science. It is not a mere metaphysical subtlety, but a subject of practical importance that I have asked you to consider to-night. For if the chemico-physical hypothesis of life be true, the only road of progress in biology lies through the chemical and physical laboratories. Now, I have already this evening more than once indicated how highly I esteem the class of biological work that has already been done in these laboratories, and I have endeavored to show how large is the unexplored biological field that can be explored only in this manner. But in addition to all that we can ever hope to do in this direction—and I insist upon its importance—I insist also upon the importance of other lines of work: I insist upon the importance of the systematic study of the phenomena of growth and development, of generation and heredity, of sensibility and mind. All that can thus be learned we need to know, and not merely for its own sake. This knowledge is indispensable to the right interpretation of the succession of life upon the globe in the past, and the successful direction of the interference of the human will with the future succession of life upon the globe in accordance with human necessities. We shall make slow progress in this direction if we confine our efforts to the application of chemistry and physics to those phenomena of living beings that can be thus explained. The other phenomena, not thus explicable, must also be studied in detail, arranged into orderly groups, and made the basis of such inductions as our

knowledge of them may warrant. It is only by pursuing this method that we can hope ultimately to acquire, with regard to the phenomena of living beings, that power to predict, which is the criterion of true science, and that power to control, which we so sorely need.

NOTES.

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[1] GEORGE F. BARKER—*Some Modern Aspects of the Life Question.* Address as President of the Amer. Ass. for the Advancement of Science. Boston meeting, August, 1880. Proceedings, Vol. XXIX, Part I, p. 23.

[2] GALEN—*Quod animi mores corporis temperamenta sequantur,* Cap. 3. [Kühn's Edit., T. IV, p. 772.]

[3] ST. GEORGE MIVART—The Cat. London, 1881, p. 387.

[4] First taught by J. R. MAYER—*Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel: Ein Beitrag zur Naturkunde.* Heilbronn, 1845.

[5] See, for example, M. FOSTER—*Text Book of Physiology,* 2d Edit., London, 1878, p. 355.

[6] BARKER—*op. cit., supra.*

[7] See H. SENATOR—*Unters. über die Wärmebildung und den Stoffwechsel,* Archiv. für Anat. Phys. und wiss. Med., 1872, S. 1.

[8] FOSTER—p. 368, *op. cit., supra.*

[9] L. LANDOIS—*Lehrb. der Phys. des Menschen,* Vienna, 1879, S. 402.

[10] L. HERMANN—*Handb. der Phys.,* Bd. I, Th. 1, S. 242.

[11] EMIL DU BOIS-REYMOND—*Unters. über thierische Elektricität,* Berlin, 1848–60, and *Gesammelte Abhandl. zur allgemeinen Muskel-und Nervenphysik,* Leipsic, 1875–77.

[12] DU BOIS-REYMOND—*Ges. Abhandl.,* Bd. II, S. 243.

[13] BERNSTEIN—*Unters. über den Erregungsvorgang in Nerven-und Muskelsysteme,* Heidelberg, 1871; also Du Bois-Reymond's Archiv, 1875, S. 526; Hermann in Pflüger's Archiv, Bd. X, 1875, S. 48.

[14] L. HERMANN—*Weitere Unters. zur Phys. der Muskeln und Nerven*, Berlin, 1867; also *Handb. der Phys.*, Bd. I, Th. 1, Leipsic, 1879, S. 192 *et seq.*

[15] HERMANNN—*Handb. der Phys.*, Bd. I, Th. 1, S. 215.

[16] ENGELMANN—Pflüger's Archiv, Bd. XV, 1877, S. 116 *et seq.*

[17] DU BOIS-REYMOND—*Ges. Abhandl.*, Bd. II, S. 319 *et seq.*

[18] MAYER—Müller's Archiv, 1854, S. 214; AMICI (1858)—Translation in Virchow's Archiv, Bd. XVI, 1859, S. 414.

[19] SCHWANN—in *Müller's Handb. der Phys.*, 1837, Bd. II, S. 59.

[20] C. B. RADCLIFFE—*Dynamics of Nerve and Muscle*, London, 1871.

[21] MATTEUCCI—*Lectures on the Physical Phenomena of Living Beings*, (translated by J. Pereira,) London, 1847, p. 333.

[22] ARTHUR GAMGEE—*A Text Book of the Phys. Chemistry of the Animal Body*, Vol. I, London, 1881, p. 418.

[23] L. HERMANN—*Grundriss der Phys. des Menschen*, 5te Aufl., 1874, S. 231.

[24] DU BOIS-REYMOND—*Ges. Abh.*, Bd. II, S. 320.

[25] FOSTER—*op. cit.*, p. 79 *et seq.*

[26] C. A. HAUSEN—*Novi profectus in historia electricitatis*, Leipsic, 1743. I cite from DU BOIS-REYMOND—*Unters. über thierische Elektricität*, Bd. II, Berlin, 1849, Th. 1, S. 211.

[27] DU BOIS-REYMOND—*Ges. Abh.*, Bd. II, S. 250.

[28] BERNSTEIN—*op. cit.*, *supra.*

[29] HERMANN—*loc. cit.*, note [14], *supra;* also *Handb. der Phys.*, Bd. II, Th. 1, Leipsic, 1879, S. 144 *et seq:*

[30] See especially DU BOIS-REYMOND—*Unters.*, Bd. II, Th. 1, S. 289, and PFLÜGER—*Unters. über die Physiologie des Electrotonus*, Berlin, 1859: An excellent summary of the observations (with the literature) is given by HERMANN—*Handb. der Physiologie*, Bd. II, Th. 1, S. 157 *et seq.*

[31] RADCLIFFE—p. 74 *et seq.*, *op. cit.*, *supra.*

[32] A. VON HALLER—*Elementa Physiologiæ*, Lib. X, Sect. VIII, § 15, T. IV, Lausanne, 1762, p. 380. He cites as authority the essay of LE CAT, crowned by the Berlin Academy in 1753. [We have in the S. G. O. Library the Berlin edition of 1765, *Traité de l'existence, etc., du fluide des nerfs, etc.*]

[33] BARKER—p. 8, *op. cit., supra.*

[34] CLAUDE BERNARD—*Leçons sur la Phys. et la Path. du système nerveux,* Paris, 1858, T. I, p. 157 and p. 224.

[35] Translation of a lecture given by E. Du Bois-Reymond at the Royal Institution, London, in Appendix No. 1 of H. BENCE JONES' *Croonian Lectures on Matter and Force,* London, 1868, p. 130.

[36] HERBERT SPENCER—*The Principles of Psychology,* Vol. I, New York, 1871, p. 95. Compare also his *Principles of Biology,* Vol. II, New York, 1867, p. 346 *et seq.*

[37] FOSTER—p. 79, *op. cit., supra.*

[38] A. GAMGEE—p. 447, *op. cit., supra.*

[39] O. HAHN—*Die Meteorite und ihre Organismen,* Tubingen, 1881. I cite the Jour. of the Royal Mic. Society, October, 1881, p. 723.

[40] "Lapides crescunt, Vegetabilia crescunt et vivunt, Animalia crescunt, vivunt et sentiunt." This phrase occcurs in the first edition of the *Systema Naturæ,* Leyden, 1735. I cite the reprint of FÉE, Paris, 1830, p. 3, as well as the second Stockholm edition, 1740, p. 76. The expression is replaced in the later editions by more guarded language.

[41] HERBERT SPENCER—*The Principles of Biology,* Vol. I, New York, 1866, p. 107.

[42] HERBERT SPENCER—*First Principles,* Amer. Ed., New York, 1864, p. 274.

[43] HERBERT SPENCER—*op. cit.,* p. 65 *et seq.*

[44] As cited by Mr. HERBERT SPENCER, *loc. cit.,* last note.

[45] GAMGEE—p. 425 *et seq., op. cit., supra.*

[46] HENRY BENCE JONES—*On the variations in the alkaline and earthy phosphates in disease,* Phil. Trans. for 1846, p. 449.

[47] MOSLER—*Beitraege zur Kentniss der Urinabsonderung,* etc., Inaug. Diss., cited in Canstatt's Jahresbericht, 1853, Bd. I, S. 134.

[48] H. BYASSON—*Essai sur la relation qui existe à l'état physiologique entre l'activité cérébrale et la composition des urines,* Paris, 1868.

[49] W. ZUELZER—*Ueber das Verhältniss der Phosphorsäure zum Stickstoff im Urin,* Virchow's Archiv, Bd. 66, 1876, S. 223.

[50] STRUEBLING—*Ueber die Phosphorsaüre im Urin*, Archiv. für exp. Path. und Pharm., Bd. VI, 1876–7, S. 266.

[51] J. S. LOMBARD—*Experiments on the relation of heat to mental work*, The New York Medical Journal, Vol. V, 1867, p. 199.

[52] J. S. LOMBARD—*Experimental researches on the temperature of the head*, Proc. of the Royal Society of London, Vol. 27, 1878, p. 166; IDEM—*The regional temperature of the head*, London, 1879; IDEM—*Experimental researches on the temperature of the head*, London, 1881. MORITZ SCHIFF—*Recherches sur l'échauffement des nerfs et les centres nerveux à la suite des irritations sensorielles et sensibles*, Archives de Physiol. norm. et path., T. III, 1870, p. 5 *et seq.* BERT—*Communication to the Société de Biologie*, read Jan. 18, 1879, in Gazette Hebdomadaire, Jan. 24, 1879, p. 63. BROCA—*Communication to the French Association for the Advancement of the Sciences*, at the Havre meeting of 1877, in Gaz. Hebd., Sept. 7, 1877, p. 577; also Gaz. Méd. de Paris, 1877, p. 457; IDEM in London Med. Record, Jan. 15, 1880. L. C. GRAY—*Cerebral Thermometry*, The New York Med. Jour., Vol. 28, 1878, p. 31; also Chicago Jour. of Nervous and Mental Diseases, Vol. VI, 1879, p. 65.

[53] See, besides the papers cited in the last note, C. K. MILLS in The New York Med. Record, Vol. 14, 1878, p. 477, and Vol. 16, 1879, p. 130; MARAGLIANO and SEPPELLI—*Studies on cerebral thermometry in the insane*, translated by J. Workman, The Alienist and Neurologist, St. Louis, Jan., 1880, p. 44 *et seq.*; R. W. AMIDON—*The effect of willed muscular movements on the temperature of the head*, Archives of Medicine, April, 1880, p. 117.

[54] FRANÇOIS FRANK—*Communication to the Société de Biologie*, May 29, 1880, in Gaz. Hebd., June 11, 1880, p. 392.

[55] ANGELO MOSSO—*Sopra un nuovo metodo per scrivere i movimenti dei vasi sanguini nell'uomo*, Atti della Reale Accademia della Scienza di Torino, T. XI, Nov. 14, 1875. I have not obtained access to the original, but find an abstract in the Archives de Phys. norm. et path., 1876, p. 175. See also BARKER, p. 12, *op. cit., supra.*

[56] BASCH—*Die volumetrische Bestimmung des Blutdrucks am Menschen*, Stricker's Med. Jahrb., 1876, S. 431. See also ROLLET in HERMANN'S *Handb. der Phys.*, Bd. IV, Th. 1, Leipsic, 1880, S. 306.

[57] MOSSO—*Die Diagnostic des Pulses in Berzug auf die localen Veränderungen desselben*, Leipsic, 1879; also by the same, *Sulla circolazione del sangue nel cervello dell'uomo*, Rome, 1880.

[58] THANHOFFER—*Der Einfluss der Gehirnthätigkeit auf den Puls*, Pflüger's Archiv., Bd. XIX, 1879, S. 254.

[59] EUGÈNE GLEY—*Essai critique sur les conditions physiologiques de la pensée.*

État du pouls carotidien pendant le travail intellectuel, Archives de Phys. norm. et path., Sept.-Oct., 1881, p. 741.

[60] BARKER—p. 11, *op. cit., supra*.

[61] HIRSCH—*Détermination télégraphique de la difference de longitude entre les observatoires de Genève et de Neuchatel*, Genève et Bale, 1864. DONDERS—in Reichert and Du Bois-Reymond's Archiv., 1868, p. 657.

[62] T. H. HUXLEY—*The connection of the Biological Sciences with Medicine*, The Popular Science Monthly, October, 1881, p. 800.

At the conclusion of the reading the thanks of the Society were voted to the President for his able and instructive address.

208TH MEETING. (11TH ANNUAL MEETING,) DECEMBER 17, 1881.

The President in the chair.

Forty-four members present.

The minutes of the last annual meeting were read and adopted.

The Secretary, Mr. THEODORE GILL, read the list of members who had been elected since the last annual meeting.

The Treasurer read to the Society his report upon the receipts, expenditures, and remaining funds of the Society for the year now about to close. He also read the list of members whose dues had been paid.

The Chair then reported to the Society a resolution of the General Committee, which is as follows:

Resolved, That the President be requested to ask the Society to appoint a committee to audit the Treasurer's report, and to communicate the result of their audit to the Society at its next meeting.

In accordance with this request, and also with that of the Treasurer, it was moved and carried that the Chair appoint a committee of three for the purpose named in the resolution.

The Chair appointed a Committee of Audit, consisting of Messrs. John Jay Knox, G. K. Gilbert, and Robert Fletcher.

Mr. THORNTON A. JENKINS then offered the following resolution:

Resolved, That all persons who have resigned membership in the Society, or failed in their duties as provided for in the rules of the

Society, shall be dropped from the succeeding published list of members.

By a vote of the Society this resolution was referred to the General Committee.

The Society then proceeded to ballot for officers for the ensuing year, and the following officers were elected:

President,	WILLIAM B. TAYLOR.	
Vice-Presidents,	J. E. HILGARD.	J. C. WELLING.
	J. J. WOODWARD.	J. K. BARNES.
Treasurer,	CLEVELAND ABBE.	
Secretaries,	THEODORE N. GILL.	MARCUS BAKER.

MEMBERS OF THE GENERAL COMMITTEE.

J. S. BILLINGS.	GARRICK MALLERY.
C. E. DUTTON.	SIMON NEWCOMB.
J. R. EASTMAN.	J. W. POWELL.
E. B. ELLIOTT.	C. A. SCHOTT.

WILLIAM HARKNESS.

The rough minutes of the meeting were then read and approved, and the Society adjourned.

209TH MEETING. JANUARY 14, 1882.

The President, WM. B. TAYLOR, in the chair.

Upon taking the chair President-elect TAYLOR offered a few remarks, and thanked the Society for the honor conferred upon him.

The minutes of the 207th meeting—the 208th being the annual meeting—were then read and approved.

A communication by Mr. BENJ. ALVORD was read, entitled

CURIOUS FALLACY AS TO THE THEORY OF GRAVITATION.

Some years since I noticed in a text book on astronomy, used in one of the most celebrated colleges in the United States, a pretended demonstration that the attraction of gravitation *must* vary inversely as the square of the distances. It was continued in several editions down to about 1850, when that portion was omitted. I always supposed that the author copied it from some old authority; that he was not guilty of inventing it, abused as it was.

In "Hind's Dictionary of Arts and Sciences" (one volume, folio, London, 1769, copy in the Congressional Library) it is found under the article "Attraction."

The first named author announced that "Gravity at different distances from the east *must* vary inversely as the square of the distances." He proceeded substantially as follows:

"The total amount of attraction exerted by the earth upon bodies exterior to it is the same as though that force was all concentrated in the centre. But a force or influence which proceeds in right lines from a point in every direction is diminished as the square of the distance is increased. For, let the centre of the earth be the vertex of a pyramid, cut said pyramid by two parallel bases at different distances from the vertex, making two similar pyramids. *Whatever the nature of gravity, its influence at the distance of each base must be equally diffused over the base. Therefore its intensity or force will be as much less at the greater base, as contrasted with its influence at the nearer and lesser base, as the surface of the latter is to the surface of the former.* But the surfaces of these bases are to each other as the squares of their distances from the vertex. Therefore the force of gravity varies inversely as the square of the distances.—Q. E. D."

Actually he placed Q. E. D. to it as if it was a mathematica, demonstration!

He afterwards said:

"The intensity of light at different distances from the radiant varies inversely as the square of the distances. This proposition is proved in the same manner as that respecting gravity, the reasoning in which applies to *all* emanations from a centre."

Subsequently, when he got to refer to the laws of Kepler, he said:

"They, therefore, became known as *facts* before they were demonstrated mathematically. The glory of this achievement was reserved for Newton, who proved that they were necessary results of the law of universal gravitation."

This sentence would have astonished Newton! It places the cart before the horse. From the empirical laws of Kepler the theory of gravitation was mathematically derived by Newton. Not the reverse. What a confusion of ideas that Kepler's laws could both be demonstrated mathematically and observed as facts? How it belittles the labors of Newton, who should have made his discovery (*de novo* from his own breast) by a geometrical process and not from the observed facts!

But my principal object in referring to this curious fallacy was to give an attempt of my own to show its fallacy by a "*reductio ad absurdum.*"

I can prove by an entirely similar process, with equal plausibility, that the force of gravity must vary inversely *as the cubes of the distances.* Instead of a pyramid take a cone. Let the centre of the earth be the vertex of a cone. Place two spheres or molecules of different sizes,* tangent to the cone, at different distances from the vertex. *Whatever the nature of gravity, its influence at the distance of each sphere must be equally diffused throughout the solid contents or volume of each sphere. Therefore its intensity or force will be as much less at the greater sphere, as contrasted with its influence at the nearer and smaller sphere, as the volume of the latter is to the volume of the former.* But these volumes or solid contents vary as the cubes of their radii, or as the cubes of their distances from the vertex. Therefore the force of gravity varies inversely as the cubes of the distances.

The oracular "Q. E. D." could have been placed to this fallacy with full as much propriety as in the former case, for I have used nearly identical words. Of course they are both pure assumptions. Neither are mathematically true, and the one destroys the other, as they are contradictory. But the first is true as arrived at by severe induction from the observed facts.

If I was a professor of logic, I should give these as specious examples of the danger of false premises, and of the ease with which they could be manufactured.

Indeed, the authors first named would imply that there could in the science of mechanics be no central forces, no empirical laws. Indeed, they would reduce the whole planetary system, the whole cosmos, to a geometrical necessity; and they would lose that interesting exposition in physical astronomy as to the wisdom and beneficence exhibited in the planetary system as it exists.

In the well-known discussion of central forces by Poisson, the equation of the curve when referred to co-ordinate axes is ascer-

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* The word molecules, being now a favorite word with the physicists, might suit the casuist a little better.

tained, and the change of one constant in the equation causes a change in the nature of the curve. If the law varied *directly as the distance*, the orbits of the planets would be ellipses as now, (but the sun would be at the centre, and not at one foci,) and they would all revolve in the same period about the sun, and on the surface of any planet no attraction towards its centre would exist. This curious result would follow: that any object projected into the air would immediately be carried from the earth, and would perpetually revolve as a satellite, like the moon, around it. All terrestrial objects would be unsettled and float about in the air in the utmost disorder.

If, on the contrary, the law varied inversely *as the cube of the distance*, (according to that precious second fallacy above set forth,) each planet would describe a spiral orbit, (if at first projected towards the sun,) continually winding and winding towards the sun; or, if perchance projected at first *from* it, would move in a spiral curve, causing it to recede farther and farther from the sun; and the eye of Omniscience alone could trace its final wanderings. What a contrast, all these suppositions, to the order, stability, beauty, and beneficence of our planetary system as it exists!

The next communication was by Mr. M. H. DOOLITTLE

ON THE GEOMETRICAL PROBLEM TO DETERMINE A CIRCLE EQUALLY DISTANT FROM FOUR POINTS.

"Describe a circumference equally distant from four given points; the distance from a point to the circumference being measured on a radius or radius produced. In general there are four solutions." (Chauvenet's Geometry, problem 110.)

These four solutions were undoubtedly obtained in accordance with the conception of three given points all either inside or outside of the required circumference. Three other solutions may be obtained from the conception of two given points inside and two outside. Mr. Marcus Baker has suggested that a distance may properly be measured from a given point through the centre of the circle to the opposite side of the circumference. This interpretation increases the number of solutions to fourteen.

This communication gave rise to a brief discussion, participated in by Messrs. HARKNESS, NEWCOMB, and BAKER, the latter pointing out that the problem appears among the exercises of Rouché and Comberousse's Traité de géométrie élémentaire, (2d ed., p. 113, Ex. 124,) a source from which Prof. Chauvenet drew many of his exercises. In Chauvenet's Geometry this problem appears as Exercise 110, page 308, with the statement that there are in general *four* solutions. This statement does not occur in the French work cited, and, therefore, the error appears to be due to Chauvenet himself, a thing somewhat noteworthy, as Chauvenet's works are in general very accurate.

Mr. ALVORD then remarked

ON SOME OF THE PROPERTIES OF STEINER'S "POWER-CIRCLE."

After the consideration of this communication the report of the Auditing Committee, appointed at the 208th meeting, was called for, and, in the absence of the chairman, Mr. Knox, was presented by Mr. Fletcher. The following is the report:

WASHINGTON, *January* 13, 1882.

Mr. President and Gentlemen
of the Philosophical Society of Washington:

We, your committee, appointed at the annual meeting, December 17th, 1881, to audit the report of the Treasurer for the years 1880 and 1881, have the honor to submit the following report:

We have examined the statement of receipts of dues from members and of interest on bonds, and find the former to be $1,175 and the latter $125, as appears in the Treasurer's statements of accounts for the years 1880 and 1881.

We have examined the vouchers for disbursements for the same period, and find them correct.

We have compared the return checks with the vouchers and with the entries in the bank book, and find them correct.

We have examined the bank book, and found the balance as set forth to be correct, said balance, deducting the amount of two checks not yet returned, being $320.16, with Messrs. Riggs & Co.

The bonds referred to in the statements of assets were exhibited to us by the Treasurer, and consist of $1,000 U. S. 4½s and $500 4 per cent. bonds.

All of which is respectfully submitted.

JNO. JAY KNOX.
ROBERT FLETCHER.
G. K. GILBERT.

The report was adopted, and the committee discharged.

The President, Mr. TAYLOR, then offered a brief communication

ON THE TOTAL LUNAR ECLIPSE OF JUNE 11, 1881.

This was noteworthy for the bright illumination of the moon's disk, which occurred during totality. The features of the moon's surface could be seen almost as distinctly during total eclipse as during full moon. This phenomenon was attributed to the refraction caused by the earth's atmosphere. To an observer stationed upon the moon a bright circle of sunlight would be visible surrounding the earth, and to the light from this source was attributed the illumination of the moon's disk seen during total lunar eclipses.

This communication was discussed by Mr. HARKNESS.

Mr. DALL then presented a brief communication

ON SOME PECULIAR FEATURES OF MOLLUSKS FOUND AT GREAT DEPTHS.

While considerable difficulty was experienced in separating some of the forms by their shells alone, yet, when their anatomy was examined, some very striking differences were presented. Among the dredgings off the Atlantic coast and in the Gulf of Mexico by the *Blake* were found mollusks claimed to be representatives of two new families having a dentition simulating that of the Docoglossa. One related to the Fissurellidæ and the other referable to the order Rhipidoglossa.

This communication was discussed by Messrs. GILL and ALVORD, after which the Society adjourned.

210TH MEETING. JANUARY 28, 1882.

President WM. B. TAYLOR in the chair.

Thirty-nine members and visitors present.

Mr. FERREL presented to the Society a communication entitled

ON THE CONDITIONS DETERMINING TEMPERATURE,

but, from lack of time, did not complete its presentation, and asked for a continuance at some future meeting.

Mr. L. F. WARD then read a paper entitled

ON THE ORGANIC COMPOUNDS IN THEIR RELATIONS TO LIFE.

This paper was briefly discussed by Messrs. ANTISELL and ELLIOTT, after which the Society adjourned.

211TH MEETING. FEBRUARY 11, 1882.

President WM. B. TAYLOR in the chair.

Mr. GILBERT presented to the Society a communication

ON ERRORS OF BAROMETRIC OBSERVATIONS PRODUCED BY WIND.

This communication will be published in full in the Report of the Geological Survey.

This communication was discussed by Messrs. BAKER, MASON, and ANTISELL, after which the Society adjourned.

212TH MEETING. FEBRUARY 25, 1882.

President WM. B. TAYLOR in the chair.

Thirty members and visitors present.

Mr. FERREL presented to the Society the concluding portion of a communication offered to the Society at its 210th meeting, January 28th,

ON THE CONDITIONS DETERMINING TEMPERATURE.

The usual formula for the rate of cooling of a heated body in vacuo, first given by Pouillet as determined from the experiments of Dulong and Petit, is of the form:

$$\delta h = Bf(\mu^{\tau} - \mu^{\tau'})$$

In which

B = the units of heat radiated by a unit of lamp-black surface in a unit of time;

f = the radiating power of the body, lamp-black being unity;

τ = the temperature of the cooling body;

τ' = the temperature of the enclosure;

μ = a constant, of which the value is 1.0077;

δh = the heat lost in a unit of time for each unit of surface.

The first part of the second member, $Bf\mu^{\tau}$, expresses the amount of heat radiated by the body, and the second, $Bf\mu^{\tau'}$, the amount of heat received from the enclosure; the radiating and absorbing powers being usually assumed to be the same, f is common to both.

In applying this formula to bodies in space, protected from the rays of the sun, τ' would represent the temperature of space, by which is meant the temperature at which a body would stand by the heat received from the stars. In applying it to bodies on the earth's surface it may be regarded as the temperature of an imaginary enclosure, from which as much heat would be received as from all surrounding objects, the earth's surface, and the atmosphere, &c., not including the sun, and hence it represents the shade temperature.

If we now suppose the body to be exposed to the direct rays of the sun, the amount of heat thus received must be added to that received from space, or from terrestrial surroundings, that is, to $Bf\mu^{\tau'}$, and the preceding formula then becomes

$$\delta h = -K\rho f + Bf(\mu^{\tau} - \mu^{\tau'}) \tag{1}$$

In which

$K =$ the units of heat received from the sun on a unit of surface;

$\rho =$ the ratio between the surface receiving rays, projected on a plane perpendicular to the rays, and the whole radiating surface.

As the body receives the rays from one direction and upon one side only, and radiates from all sides, the average amount of heat, $K\rho f$, received over the whole surface and absorbed, must be compared with the amount lost by radiation, and hence the factor f must come in, since only the heat absorbed affects temperature, the absorbing and radiating power here, as usual, being assumed to be the same.

In the case of a spherical body, as the bulb of a thermometer, the value of ρ becomes $\frac{1}{4}$, since the projected receiving surface of the sphere is one-fourth of the whole radiating surface of the sphere. In the case of a long cylinder, in which the radiation from the ends could be neglected in comparison with the whole, the value of ρ becomes $\frac{1}{\pi}$, if the side of the cylinder is exposed perpendicularly to the sun's rays. In the case of a thin disk, with its surface perpendicular to the sun's rays, neglecting the radiation from the edge, the value of ρ would be $\frac{1}{2}$. In the case of such a disk, in which the radiation is from one side only, which would be approximately so in the case of such a disk with the opposite side of polished silver, the value of ρ would be unity.

The amount of heat, K, received from the sun through the atmosphere at the earth's surface is usually expressed by

$$K = Ap^{\varepsilon} \tag{2}$$

In which

A = the heat received from the sun on a unit of surface at the top of the atmosphere;

ε = the secant of the zenith distance of the sun;

p = a constant for all zenith distances, but differing in different states of the atmosphere, but always less than unity.

In the case of a static equilibrium of temperature, which was the only case considered, δh vanishes, and the preceding equations, (1) and (2), give

$$\rho A p^{\varepsilon} = B(\mu^{\tau} - \mu^{\tau'}) \tag{3}$$

This equation expresses the condition which determines the static temperature, τ, of a body, and it is seen that this depends upon the solar constant A; the form of the body, upon which the value of τ depends; upon the value of p, or the state of the atmosphere; upon the zenith distance, which determines ε; upon the radiating constant, B; and upon the shade temperature, τ'.

Putting for the unit of heat the amount required to raise the temperature of a cubic centimetre or grain of water one degree centigrade, and the square centimetre, second, and degree centigrade, for the units of surface, time, and temperature, respectively, the value of B was determined by the author, from the experiments of Mr. J. P. Nichol on the rate of cooling of a blackened copper ball in vacuum, surrounded by an enclosure of blackened surface, (Proc. Royal Soc. Edin., 1869–70, p. 207,) to be .01808. This value was considered more reliable than that of Pouillet from the experiments of Dulong and Petit, since the latter were made on the rate of cooling of mercury in a glass bulb, and the results had to be reduced to those which would have been obtained with a blackened surface; and the value of the radiating power, f, for glass, which was used in this reduction, Pouillet states, was somewhat hypothetical, and so it left some doubt with regard to the true value of the constant. Pouillet's value of B for the minute-unit was 1.146, and this reduced to the second-unit is .01910. The value $\mu = 1.0077$ required no change to satisfy the results of Mr. Nichol's experiments.

The value of A, deduced from the experiments of Pouillet and Herschel with the actinometer, is .03046 for the mean distance of the sun, both sets of experiments, when reduced to the sun's mean distance, giving very nearly the same value. At the time of the earth's perihelion this is about one-thirtieth greater, and at aphelion as much less.

Pouillet's value of p for clear weather is about 0.75, but others make it considerably less. It can hardly be regarded as a constant, but only as a sort of average of values for clear weather, which may differ very much at different times. According to Tyndal, who maintains that the absorption power of the atmosphere in clear weather depends almost entirely upon the amount of aqueous vapor in it, the value of this constant, even in clear weather, must depend very much upon the hygrometric state of the atmosphere.

With the preceding numerical values of the constants of A and B, the preceding equation gives

$$\mu^{\tau - \tau'} = \frac{1.685\, \rho p^{\epsilon}}{\mu^{\tau'}} + 1 \tag{4}$$

for determining the value of $\tau - \tau'$, for any zenith distance of the sun, of which the secant is ϵ, where the value of p and the shade temperature τ' are known. But since the value of B was determined for a vacuum, this formula is only applicable where the radiating body is in a vacuum, and cannot be applied in cases where the body receives or loses heat by conduction or connection.

The first term of the second number of the preceding equation depends upon K, the heat received from the sun, and, therefore, vanishes where the body is in the shade, and we then have $\tau - \tau' = 0$. Hence the temperature of all bodies having the same surroundings must cool down to the same temperature, τ'. This is a necessary consequence of the equality of the absorbing and radiating powers of bodies.

The author had been able to find but few observations of the value of $\tau - \tau'$ to compare with the theoretical value given by the preceding formula. Hooker states that from a multitude of desultory observations made on the Himalaya Mountains at an elevation of 7,400 feet, he concluded that the average effect of the sun's rays on a black-bulb thermometer was 125.7° or 67° (37.2° C.) above the temperature of the air. The shade temperature was, therefore, 14.8° C. With this value of τ', and the value $\rho = \frac{1}{4}$ for the spherical bulb, we get $\tau - \tau' = 41.6°$ at the top of the atmosphere where $p = 1$. The value of p for that altitude, and also the value of ϵ for the observations, are not accurately known. At the elevation of 7,400 feet, Pouillet's value of $p = .75$ would have to be considerably increased, but the effect of the exponent ϵ would perhaps bring the value of p^ϵ equal to about .75. With this value of p^ϵ the formula gives $\tau - \tau' = 32.4°$, five degrees too small for the observed value.

Again, at the height of 13,100 feet, he found in January, at 9 a. m., the temperature of the black bulb 98° with a difference of 68.2°, and at 10 a. m., 114° with a difference of 81.4°. From the average of these we get $\tau' = - 0.4°$ C. and $\tau - \tau' = 41.6°$ C. The preceding formula gives $\tau - \tau' = 45.7°$ C. at the top of the atmosphere where $p = 1$. At the elevation of 13,100 feet the value of p^ϵ should not be very much less than unity—perhaps about as much less as would reduce the theoretical value 45.7° down to the observed value 41.6°.

It should be remarked here that the theory requires that the two thermometers should have exactly the same surroundings. If the one thermometer is in a vacuum surrounded by a glass bulb and the other outside, this condition is not perfectly fulfilled, and the indication of the thermometer outside in the shade might vary a little from one in the shade within the bulb, unless this bulb is so situated as to have the same temperature as the external shade thermometer.

If, in place of a black-bulb thermometer, we had a thin disk with a blackened side exposed perpendicularly to the sun's rays, and the opposite side of polished silver of which the radiating power is extremely small, we should have in this case the value of $\rho = 1$ very nearly, and with this value of ρ the formula would give, in the first of the examples above, for the top of the atmosphere, $\tau - \tau' = 106.6°$ C., which, added to the shade temperature, 14.8°, would give $\tau = 121.4°$ C. This enormously high temperature is not inconsistent with observation, for water has been made to boil from the effect of the direct rays of the sun at the earth's surface, where the theoretical condition of our formula, that no heat shall be lost by conduction, was not perfectly fulfilled.

A portion of the earth's surface, where the soil is dry and sandy, having little conductivity for heat and exposed to the vertical rays of the sun, would be a case similar to that of an isolated disk radiating sensibly from one side only, and the temperature of such a surface, so exposed, should stand at a very high temperature, but of course not nearly up to the theoretical temperature, since much heat would be conveyed away by the conduction and connection of the air, and also some conducted down into the earth. The temperature of sandy soils is often observed to be as high as 150° F. and upwards, and the preceding theory explains these very high temperatures and the great differences of temperature of different bodies under the same circumstances.

From equations (2) and (3), with the given values of A and B, we get

$$K = .07232\,\mu^{\tau'}(\mu^{\tau-\tau'} - 1) \tag{5}$$

This is an actinometric formula, giving the amount of heat received from the sun, in absolute heat units, from the observation of ihe sunshine and shade temperatures. So far as the author's reading extends no such formula has ever been given, but $\tau - \tau'$ has been regarded as a measure of the sun's relative intensity under different circumstances. The formula not only gives the absolute instead of the relative amount of heat received, but it shows that $\tau - \tau'$ is not proportional to K, and consequently not a correct measure of the relative intensities of the sun's rays. With an observed value $\tau - \tau' = 35°$ and $\tau' = 30°$ the formula gives $K =$ .02806; but with the same value of $\tau - \tau'$, and with the value of $\tau' = 0°$, it gives $K = .02229$. Hence the value of K is not proportional to $\tau - \tau'$, and differs considerably when the value of $\tau - \tau'$, under different circumstances, is the same. Both these values of K are less than the value of $A = .03046$, as they should be by equation (2). The greater the altitude the more nearly should the value of p approximate to that of unity, and the more nearly should the value of K approximate to that of A.

If the value of p, according to Tyndal, as has been stated, depends upon the hygrometric state of the atmosphere, then the value of K, as given by the preceding formula, for any observed values of τ and τ', must give the diathermancy, and consequently the hygrometric state of the atmosphere in clear weather, not only for the point of observation, but generally throughout the whole extent of the atmosphere through which the rays pass, for the greater the value of K the greater the diathermanancy of the air, and hence the less the amount of aqueous vapor in it.

This was briefly discussed by Messrs. HARKNESS, H. FARQUHAR, and TAYLOR.

Mr. ANTISELL then began the presentation of a communication

ON THE BUILDING UP OF ORGANIC MATTER,

which was unfinished when the hour of adjournment arrived, and its completion went over to the next meeting.

213TH MEETING. MARCH 11, 1882.

President WM. B. TAYLOR in the chair.

Thirty-seven members and visitors present.

Mr. ANTISELL then presented to the Society the remainder of his communication

ON THE BUILDING UP OF ORGANIC MATTER,

the presentation of which was begun at the last meeting.

A brief discussion of this paper—the session having been prolonged for this purpose—followed, and was taken part in by Messrs. GILL and WARD, who took exceptions to some of the conclusions arrived at in the communication.

214TH MEETING. MARCH 25, 1882.

President WM. B. TAYLOR in the chair.

Thirty-six members and visitors present.

The President announced to the Society the death, at 3 p. m. this day, of pneumonia, after an illness of two days, of Mrs. Joseph Henry, widow of the first president of the Society.

Mr. A. B. JOHNSON then presented to the Society a communication

ON SOME PECULIAR RAVAGES OF TEREDO NAVALIS.

This communication was discussed by Messrs. ANTISELL, DALL, GILL, HARKNESS, and WHITE.

Mr. ANTISELL called attention to the fact that the existence of the Teredo, as well as that of other destructive mollusks brought to our harbors by shipping, along our entire coast is well known, and that, in view of this fact, it is a matter of surprise that provision was not made for guarding against this danger. To this it was answered by Mr. Johnson that the wharf was a temporary one, being only needed for three months, and that, although the presence and destructive powers of the Teredo were recognized by the Board, it did not appear that in any previous case the destructive action of the Teredo was so rapid as to render special precaution necessary in this case. Upon a question from Mr. Harkness it was asserted by Mr. Johnson that a pile, examined on September 15 by divers, and found sound—chips cut by divers from the pile under water were found unbored by the Teredo—broke down on September 19, thus indicating a destruction of a pile in four days.

The accuracy of the observation of September 15, that the chips were unbored, was questioned by Mr. Dall, who asserted that the Teredo in its youngest stage attacks the wood, and that the hole made is at first very minute, and is gradually enlarged and deepened as the mollusk grows. So that a pile which appears sound on the surface may, in fact, already be seriously injured by Teredo borings. In San Francisco Bay the work of destruction of piles by the Teredo, and their renewal goes on continually, and it is estimated that a complete renewal of all the piles in the bay occurs every seven years. The mollusk works and breeds the year round in waters above a temperature of 60° F. It attacks the hard woods, as lignum vitæ, quite as readily as softer woods, but the destruction in such case is less rapid. Such woods, however, as palmetto, consisting of bundles of tough fibres interspersed with soft or spongy material, are only slightly, if at all, injured.

Mr. GILL called attention to the fact that the Dutch Commissioners, appointed in consequence of the great ravages of the Teredo on the coast of Holland in about 1859, found creosote the best preventive. They further found that the activity of the Teredo was, to a certain extent, dependent upon meteorological conditions since the years 1720, 1755, 1782, 1820, and 1850, were seasons of great drought, and consequent increase of salinity of the sea-water along the coast, and in those years the destruction caused by the Teredo was unusually great.

Respecting the geological age of the Teredo, Mr. WHITE exhibited to the Society fossilized wood from the cretaceous formation showing Teredo borings.

Mr. BILLINGS then presented to the Society a communication

ON THE VENTILATION OF THE HOUSE OF REPRESENTATIVES,

which was unfinished when the hour of adjournment arrived, and went over to the next meeting.

Adjourned.

215TH MEETING. APRIL 8, 1882.

President WM. B. TAYLOR in the Chair.

Forty-eight members and visitors present.

Mr. BILLINGS then continued the presentation of the communication begun at the last meeting

ON THE VENTILATION OF THE HOUSE OF REPRESENTATIVES,

of which the following is an abstract:

The difficulties to be overcome, and the means used for this purpose were explained, and plans and sections of the Hall of the House of Representatives at the Capitol, in Washington, were shown. The amount of fresh air required is about one foot per second per person, if an approach to perfect ventilation is desired. The imperfect form of ventilation by dilution requires from forty to fifty feet per minute. When a hall is occupied only one or two hours, the cubic space is important, but in long sessions it is the supply rather than the space that must be looked to.

To produce the requisite movement of the large amount of air used, special force must be supplied. This may be propulsion—the plenum method, or by aspiration—the vacuum method, or a combination of the two. The effect of wind and rain on aspirating systems was alluded to. In the majority of such halls the plenum system, by means of a fan, is used. The difficulty in introducing this large amount of air into a hall depends partly on the necessity for avoiding unpleasant currents, and partly on the cost of heating and supplying power. The question of cost, however, in such halls as are referred to, is usually a minor consideration, but if the tastes of individuals as to temperature are to be consulted—that is, if each man is to have his air at the temperature which suits himself—the cost becomes a serious matter.

The effects of various positions of fresh air inlets were pointed out, and stated to depend largely upon the tendency of air to adhere to surfaces over which it passes, as shown by the investigations of Savart and others. The difference between the upward and downward system were pointed out.

The various modes of heating were described, more especially with reference to their effect upon the air, and the influence of moisture was discussed. Probably the importance of moistening the air is less than has been supposed, and the methods employed for this purpose have been beneficial only indirectly.

The system of heating and ventilation of the Hall of the House was then described, and compared with that of the English Houses of Parliament, the Chamber of Deputies at Versailles, and the Grand Opera House at Vienna, and Frankfort on Main.

The great importance of skilled superintendence was pointed out, and the necessity for continuous records was insisted on.

Remarks upon this communication were made by Messrs. ANTISELL, ELLIOTT, MUSSEY, and POWELL.

Mr. HILGARD then presented a communication

ON SIEMENS' DEEP SEA THERMOMETER AND CARRÉ'S ICE MACHINE.

Remarks on this communication were made by Messrs. ANTISELL, DALL, DUTTON, and E. J. FARQUHAR, after which the Society adjourned.

216TH MEETING. APRIL 22, 1882.

President WM. B. TAYLOR in the chair.

Thirty-six members and visitors present.

The Secretary read a list of names of persons who had been elected to, and had accepted membership in, the Philosophical Society, viz: EZRA WESTCOTT CLARK, HENRY FLAGG FRENCH, HENRY ALLEN HAZEN, CHARLES HUGO KUMMEL, ISRAEL COOK RUSSELL, WILLIAM WIRT UPTON, ALBERT LOWRY WEBSTER.

Mr. FERREL then presented to the Society a communication

ON SOLAR RADIATION AT SHERMAN, WYOMING.

The next communication was by Mr. C. A. WHITE

ON ARTESIAN WELLS ON THE GREAT PLAINS.

This communication has been essentially reprodnced with the title, "Artesian Wells upon the Great Plains," (subscribed C. A. White,) in the American Review for August, 1882, No. 135, pp. 187–196.

Mr. ANTISELL called attention to previous attempts on the part of the Government to obtain water on the great plains by boring artesian wells. During the surveys and explorations of the 39th parallel, for the purpose of ascertaining the feasibility of building a railroad to the Pacific Ocean, special attention was given to the matter of obtaining water by means of artesian wells, and at that time he reached the same conclusion essentially as that now presented by Mr. White. Mr Antisell's published report upon this subject may be found in volume 7 of the Pacific Railroad Reports published in 1854.

Mr. MUSSEY called attention to boring now in progress along the line of the Southern Pacific Railroad in New Mexico; boring being in progress at the expense of the railroad company for the purpose of supplying water for locomotive purposes.

Mr. GILBERT considered the argument conclusive as to the failure of artesian wells on the great plains to be of any practical value for irrigating purposes, but for some other uses, such as stock raising, farm uses, etc. Some wells in favorable localities had proved a success, and others would also undoubtedly prove successful. Geological prophecy is generally, however, to be made with great caution, and to be received with caution equally great, a proposition which was supported by citing several cases in the experience of himself and others.

On the close of this discussion Mr. ELLIOTT presented a communication

ON THE CREDIT OF THE UNITED STATES, PAST, PRESENT AND PROSPECTIVE.

This communication will be published in another form.

Remarks upon this paper were made by Messrs. GILL and W. B. TAYLOR, after which the Society adjourned.

217TH MEETING. MAY 6, 1882.

President WM. B. TAYLOR in the chair.

Twenty-eight members and visitors.

The President announced to the Society the death of two of its members, Mr. WILLIAM J. TWINING, Major U. S. Engineers and Commissioner of the District of Columbia, and Mr. JOHN RODGERS, Senior Rear Admiral U. S. Navy and Superintendent U. S. Naval Observatory. He further announced to the Society that the proposition for a federation of the Anthropological, Biological, and Philosophical Societies had been discussed by the General Committee, but that thus far no action had been taken.

The first communication was by Mr. ELLIOTT COUES,

ON THE POSSIBILITIES OF PROTOPLASM.

The following is an abstract of this communication, which has been published at greater length under the title—"Biogen: a Speculation on the Origin and Nature of Life. Abridged from a paper on the 'Possibilities of Protoplasm,' read before the Philosophical Society of Washington, May 6th, 1882. By Dr. ELLIOTT COUES. Washington: Judd & Detweiler, printers and publishers. 1882." (8vo., pp. 27.)

Referring to previous papers on the subject of Life, by Mr. WOODWARD and Mr. WARD, the speaker opposed any purely chemico-physical theory, and adhered to the doctrine of the actual existence of a "vital principle." Granting that all substances, including protoplasm, have been evolved from nebulous matter; that evolution to the protoplasmic state is necessary for any manifestation of life, and even that life necessarily appears in matter thus elaborated, it does not follow that the result of the processes by which matter is fitted to receive life is the *cause* of the vitality manifested. For all that is known to the contrary protoplasm and vitality are simply concomitant; or if there is any causal relation between them, vital force is the cause of the peculiar properties of protoplasm, not the result of those properties. There really exists a potency or principle called "vital," in virtue of which the chemical substance called protoplasm manifests vitality, that is to say, *is alive*, and in the absence of which no protoplasmic or other molecular aggregation of matter can be alive. The chemico-physical theory simply restates abiogenesis or "spontaneous generation," of which we know nothing scientifically. The grave doubt that "life is a property of protoplasm" will persistently intrude until some one shows what is the chemico-physical difference between living and dead protoplasm; none being known.

Noting that chemistry and physics had combined to manufacture an egg which would do everything to be expected of an egg, except to hatch, the speaker summed his charge thus: The atheistic physicist, denying mind in nature, declares that matter alone exists. Matter in motion is all there is; the cosmos being matter in motion in virtue of material forces alone. This is simply to invent a kind of perpetual motion machine, and leave out even the inventor; for such a machine invented itself and set itself going. Then the materialistic chemist takes this self-started machine and declares it has laid an egg that will hatch. On any such theory a God is not only superfluous but impossible. Yet the result of the alleged self-evolution of self-created matter through chemical elements to organic compounds has been the creation of a protoplasmic soul so constituted that it must believe in a God; and if matter be that God, matter contradicts itself, for the constitution of the human soul requires that its God must be other than its protoplasmic self; while if matter be not that God, there must be some other.

The speaker argued for the existence of the soul as something apart from and unlike matter, defining "soul" as that quantity of spirit which any living body may or does possess. No idea can attach to the term "spirit" from which all conceptions of matter are not absolutely excluded. Spirit is immaterial, self-conscious force; life consists in the animation of matter by spirit.

The substance of mind and the substance of matter were noted as equally hypothetical. To the former was given the name

Biogen, or "soul-stuff," and it was defined as spirit in combination with the minimum of matter necessary to its manifestation. The analogy between biogen and luminiferous æther, or the hypothetical substance of light, was discussed. The drift of the speaker's speculation on the vital principle as an ens realissimum was toward a restatement, in scientific terms, of the old *anima mundi* theory. Modern materialistic and atheistic notions about life were denounced as every one of them disguises of the monstrously absurd statement that a self-created atom of matter could lay an egg that would hatch.

The whole matter being beyond the scrutiny of the physical senses is remote from the scope of exact science; but it is irrational and unscientific to deny it, as is virtually done when science excludes it from any share in life-phenomena, by presuming to explain life upon purely material considerations. No chemico-physical theory of life is tenable that does not satisfactorily explain the chemico-physical difference between, for example, a live amœba and a dead one; an explanation which has never yet been, and probably cannot be, given.

A general discussion of the points involved in this paper followed. Mr. POWELL pointed out what he regarded as a fundamental and fatal error in the reasoning, viz., that the axiom that the whole equals the sum of all its parts, had been assumed throughout to be true *qualitatively* as well as quantitively. Furthermore, he maintained that logical consistency required that those who believe in force should believe also in the vital principle, and *vice versa*. As for himself, however, there was neither force nor vital principle, but only matter in motion. Three relations are always to be borne in mind, viz., quantity, quality, and succession, whereas the physicist falls into error by considering only the quantitive relation.

So much of the support of the views of Mr. Coues as might be derived from the common consensus of mankind was criticised by Mr. Gill as unsound, since the common consensus of mankind has often been found at fault; the supposed flatness of the earth, the motion of the sun around the earth, etc., are examples where this criterion fails. Paraphrasing an eminent philosopher's dictum, he thought there was a tendency of biologists ignorant of philosophy and philosophers ignorant of biology to make a distinction between organic and inorganic matter, and call in a "vital force." He likened living and dead protoplasm to an electric battery in action and at rest, and maintained that life is a property of matter, and that it cannot be conceived of separated from matter.

Mr. HARKNESS avowed his belief in force, and hence in vital force, and further in a little religion, and was, therefore, moved to make inquiry concerning the chemical difference between living and dead matter.

Mr. WARD pointed out that very diverse views were held upon this subject by two classes of thinkers who do not come into intellectual contact. Furthermore, while not asserting that a belief in vital force was a superstition, attention was drawn to the fact that infantile races attribute all phenomena to supernatural agencies, and that, with increasing knowledge, there is a decrease in the number of these appeals to supernatural agencies.

The corner stone of modern science, said Mr. DOOLITTLE, is *measure.* We must have a biometer. What electrical science would be without ohms, astronomy without graduated circles, chemistry without the balance, such is biology without a *measure.* Is there more life in two mice than in one mouse? In a horse than in a mouse? Until we can answer these questions substantial progress in biology is not to be expected.

The term automatic, as used here, he considered a confession of biologic ignorance. Automatic motion, as used in the discussion, seemed to mean simply motion which cannot be relegated to any known law.

After some further desultory discussion the Society adjourned.

218TH MEETING. MAY 20, 1882.

President WM. B. TAYLOR in the chair.

Thirty-two members and visitors present.

A series of resolutions concerning the death of Admiral JOHN RODGERS, a member of this Society, which resolutions had been adopted by the General Committee, were read by the Secretary; after which Prof. CHARLES W. SHIELDS, of Princeton College, read to the Society a communication

ON THE PHILOSOPHICAL ORDER OF THE SCIENCES.

This communication has been published by Scribner's Sons in a volume entitled "The Order of the Sciences: An Essay on the Philosophical Classification and Organization of Human Knowledge." By Charles W. Shields, Professor in Princeton College. 103 pp., 12mo. New York, Charles Scribner's Sons, 1882.

This communication was discussed by Messrs. WARD, POWELL, ANTISELL, TAYLOR, ALVORD, and BAKER.

219TH MEETING. JUNE 3, 1882.

President WM. B. TAYLOR in the chair.

Twenty-two members and visitors present.

The first communication offered was by Mr. ALVORD

ON THE COMPASS PLANT.

This communication has been published with the title "On the Compass Plant," by Benjamin Alvord, in the American Naturalist for August, 1882, No. 16, pp. 625–635.

Remarks were made on the exhibition of polarity in other vegetable types by Messrs. HENRY FARQUHAR and THEODORE GILL.

Mr. E. B. ELLIOTT next presented to the Society a communication

ON SOME FORMULÆ RELATING TO GOVERNMENT SECURITIES.

Mr. C. H. KUMMELL then presented a communication

ON COMPOSITION OF ERROR FROM SINGLE CAUSES OF ERROR.

This was unfinished when the hour of adjournment arrived, and its completion went over to the next meeting.

Adjourned.

221ST MEETING. JUNE 17, 1882.

President WM. B. TAYLOR in the chair.

Twenty-three members and visitors present.

Mr. C. H. KUMMELL continued his communication

ON COMPOSITION OF ERROR FROM SINGLE CAUSES OF ERROR.

which was begun at the last meeting.

This paper is expected to appear in full in the Astronomische Nachrichten.

Remarks upon this paper were made by Messrs. E. B. ELLIOTT and W. B. TAYLOR.

Mr. MARCUS BAKER then presented the following communication

ON A GEOMETRICAL QUESTION RELATING TO SPHERES.

On January 17, 1882, Mr. Doolittle called the attention of the Society to the geometrical problem *To determine a circle equally distant from four given points in a plane*, and showed that the statement in Chauvenet's Geometry, (p. 308, Ex. 110,) that this problem admits of *four* solutions is erroneous, there being in general *fourteen* solutions. The extension of this problem to spheres and five points in space is nearly as simple as for the case of circles and four points in a plane.

Let it be proposed to solve the following:

PROBLEM.—*To determine a sphere equally distant from five given points.*

The distance to a sphere, considered here, is to be measured along a diameter, produced if necessary, and hence for any position we have two distances, one a maximum, the other a minimum.

Solution.—Case I. Through any four of five given points, a, b, c, d, e, as, for example, b, c, d, e, describe a sphere; the fifth point, a, will in general fall within or without this sphere, of which call the radius R and centre C; also, let α be the distance from the centre of this sphere to the point a. Then two spheres described with centre C and radii $\frac{1}{2}(R \pm \alpha)$ fulfil the condition of being equidistant from the five points.

Every distinct group of four of the five given points in like manner gives two solutions; hence of this kind there are in all *ten* solutions.

Case II. Through any three of the five given points, a, b, c, d, e, as a, b, c, pass the circumference of a circle; from the centre of the circle erect a perpendicular. This perpendicular is the locus of all points equidistant from points a, b, c. Join the points d and e by a line; bisect this line by a plane perpendicular thereto. This plane is the locus of all points equidistant from d and e. The intersection of these two loci is the centre of two spheres equidistant from the five points.

Every distinct group of three of the five given points in like manner gives two solutions; hence of this kind there are in all *twenty* solutions.

Therefore, in general there are *thirty* spheres equally distant from five given points.

The next communication was by Mr. H. A. HAZEN

ON THE RETARDATION OF STORM CENTRES AT ELEVATED STATIONS, AND HIGH WIND AS A PROBABLE CAUSE.

In the absence of Mr. Hazen the following abstract was read by the Secretary, Mr. Baker:

In his tenth paper, published in the January, 1879, number of the American Journal of Science, Prof. Elias Loomis advanced certain evidence, based on barometric observations, to show that apparently the progress of a storm centre was much more rapid at the surface of the earth than at elevations above it. It is the purpose of this article to put forth certain facts which, it is hoped, will tend to elucidate the subject.

Not long since, before this Society, Prof. G. K. Gilbert showed that a high wind had a tendency to depress the barometer column, as determined from his discussion of certain observations made by the Signal Service at the summit and along the side of Mount Washington, New Hampshire. If now a wind can produce such a depression, it would seem as if the wind accompanying a storm and continuing its force at a high station some time after the passage of the storm centre at the base, might cause the apparent retardation.

It is very desirable that special experiments be made, under natural conditions, directly testing the influence of high winds on the barometer column.*

It seems possible to indirectly ascertain such influence from a barometric computation of the height of a mountain by means of observations taken during different wind velocities. Table I gives such a computation of the height of Mount Washington from observations at the base and summit in May, 1872 and 1873.

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*Direct experiments have been made, using a blower for the air current, and an air-tight receiver for the barometer, at short distances, a condition of things, however, which can never occur in nature.

TABLE I.

Mean amount to be added to the true difference of elevation between the summit and base of Mount Washington in order to give the computed difference, arranged according to the force of the wind.

	Wind Force in Miles per Hour.													
	0 to 10.		11 to 20.		21 to 30.		31 to 40.		41 to 50.		51 to 60.		Above 61.	
	Cases.	Am't.	C.	A.	C.	A.	C.	A.	C.	A.	C.	A.	C.	A.
		′		′		′		′		′		′		′
May, 1872	77	−27.1	25	−18.6	30	−3.1	43	+13.8	65	+10.5	32	+33.9	50	+51.4
May, 1873	104	−43.5	134	−22.0	183	+4.1	135	+15.6	99	+34.9	61	+52.4	27	+80.1

In the above table, for May, 1872, all winds under 10 and above 40 are included, and in May, 1873, all the cases, except a few which were omitted because of serious errors in the observations.

The table shows this remarkable peculiarity that, though with winds above sixty-one miles per hour, the mean computed difference in height is too great by sixty-six feet; with winds under ten miles per hour the mean difference is too small by thirty-five feet. We conclude, then, that some other cause must produce the results, or must act in conjunction with the wind. Taking the wind above sixty-one miles per hour I have found ten cases in which the height was too small by about fifteen feet, also a great number of cases in which, though the wind continued strong from the same direction, yet the computed height continually became less, showing that the wind does not produce a direct effect upon the indications of the barometer. On projecting the curves of pressure we find that there is a uniformity in the occurrence of small and large differences of elevation with the maxima and minima of pressure, the least being found when the pressure is high, and the greatest when it is low.

Grouping a second time, then, with respect to the maxima and minima of pressure, we have Table II.

Table II.

Mean amounts to be added to the true difference of height between the summit and base of Mount Washington to obtain the computed difference.

Date.	Locality.	Maxima of Pressure.		Minima of Pressure.	
		Cases.	Amount.	Cases.	Amount.
			′		′
May, 1872	Mt. W. and base	81	— 32.5	70	+ 57.4
May, 1873	Mt. W. and base	102	— 61.6	137	+ 67.3
Jan., Feb., Mar., Oct., Nov., Dec., 1880.	Mt. W. and mean of B. and P.	119	— 29.1	120	+ 127.0

As the first two horizontal rows of figures apply only to observations for the month of May, and as it would be very desirable to have results for the colder months when the fluctuations are much increased, I have added a third set of figures for the summit of Mount Washington, compared with the mean of Burlington and Portland as the base, and computed the difference of elevation from observations taken at 7 a. m., 3 p. m., and 11 p. m., Washington time, during January, February, March, October, November, and December, 1880.

It is evident from Table II that during the prevalence of relatively high pressure, elevations computed barometrically will, in general, be too small, and, on the other hand, when the pressure is low, the computed heights will be too great. This also explains the coincidence of too great computed heights with high winds, for the reason that the highest winds always occur with relatively low pressure; on the contrary, when the wind is light, the pressure is generally high.

May not this retardation be due to the effect of varying temperature? When a "low" has passed a station at sea level the temperature frequently falls steadily, thus contracting the atmosphere and causing its withdrawal from the upper regions, and a still further fall in pressure there. This process will continue until the fall caused by the low temperature is counterbalanced by the rise due to the advancing "high." The following is given as an illustration:

Observations of air-pressure and temperature at Denver and Pike's Peak, Colorado, in November, 1880.

Day.	Hour. Wash. Time.	Temp. Pike's Peak.	Mean Temp. Pike's Peak and Denver.	Pressure.	
				Pike's Peak.	Denver.
		°	°	"	"
14	7 a. m.	−5	6	17.75	24.69
	3 p. m.	+2	20	17.75	24.64
	11 p. m.	6	19	17.82	24.59
15	7 a. m.	10	22	17.83	24.50
	3 p. m.	14	34	17.71	**24.28**
	11 p. m.	11	16	17.57	24.48
16	7 a. m.	1	6	17.28	24.41
	3 p. m.	−6	1	17.18	24.44
	11 p. m.	−14	−6	17.22	24.58
17	7 a. m.	−31	−20	**17.13**	24.54
	3 p. m.	−19	−10	17.25	24.49
	11 p. m.	−16	−12	17.42	24.42
18	7 a. m.	−9	−6	17.48	24.33
	3 p. m.	−4	7	17.41	24.23
	11 p. m.	−5	6	17.32	24.08

From these observations we see that, although the air-pressure was at a minimum at Denver, November 15, 3 p. m., yet, owing to the extraordinary cold, the pressure continued to fall at Pike's Peak, (which is 8,840 feet above Denver,) and did not reach its lowest point until forty hours afterward, or November 17, 7 a. m. Extending the same reasoning to the diurnal range of air-pressure we shall find a satisfactory solution of the retardation. From hourly observations at the summit and base of Mount Washington I find that while the morning maximum occurs at 8:30 a. m. at the base, it does not occur till noon at the summit, during this part of the day the temperature is rising rapidly; and hence we may suppose that it produces the continued rise in air-pressure at the summit overbalancing the diurnal range; in like manner the afternoon minimum occurs at 6 p. m. at the summit, or two hours later than at the base, as the temperature begins falling at 2 p. m. This may account for the difference at the two stations. On comparing the night maximum and morning minimum I find little or no retardation; this is what we might expect from the fact that at this time there is little or no change in temperature.

The President, Mr. TAYLOR, called the attention of the Society to the remarkable halo witnessed by many people in Washington last Thursday, June 15, saying that in some respects it was remarkable, and presented some theoretical difficulties. While it had been seen by a number of those present, none had made any scientific observations of it or taken any measurements. A number of other halos were mentioned which, like this, occurred between 10 and 11 a. m., and it was thought worth while to consider whether halos appeared oftener at those hours than at others, and if so, why.

221ST MEETING. OCTOBER 7, 1882.

The President in the chair.

Forty-one members present.

The consideration of the minutes of the last meeting was postponed.

The PRESIDENT welcomed the members to a renewal of the meetings of the Society after the summer vacation.

He also announced that vacancies had been created in the Committee by the resignation of Dr. J. J. Woodward, a Vice-President of the Society, on account of prolonged illness, and of Mr. Marcus Baker, one of the Secretaries, by reason of assignment to duty in California. The General Committee had elected Mr. E. B. Elliott a vice-president in place of Dr. Woodward, and Dr. J. S. Billings a secretary in place of Mr. Baker. The vacancies resulting therefrom in the membership of the Committee had been supplied by the election of Dr. D. L. Huntington, U. S. A., and Prof. C. V. Riley.

Mr. A. S. CHRISTIE made a communication

ON A SYSTEM OF STANDARD TIME.

A prime meridian (say Greenwich) time would, in general, give the hours of the local natural day dissymmetrical with respect to the zenith of the clock face and the zero point of the hour numbers. Turning the dial plate until the prime meridian hour of local mean noon comes to the zenith, eliminates the first mentioned element of dissymmetry, and is a partial adaptation of prime meridian time to local convenience. The second element of dissymmetry is inherent in the nature of numbers, and cannot be eliminated whilst they are retained; for symmetry demands that the zero point shall be either *everywhere* or *nowhere*, neither of which conditions can be satisfied by the symbols now in use. Rejecting them, therefore, and adopting a series of hour symbols having no absolute numerical, but only an ordinal, significance, is another and final step in the adaptation of prime meridian time (such only as to the hour-zero) to general use.

A consideration of what symbols to adopt will immediately suggest, that an abandonment of the artificial, and a return to the simplicity of nature, constitutes the real and complete solution of the problem. That problem may now be stated: To avoid the discordance of local time on different meridians (a discordance which cannot be removed) by the adoption of the same standard time on all meridians, so that the hour and fraction of the hour shall be the same at the same instant everywhere; which standard time shall be marred by no dissymmetry with respect to the globe, alien in no land, essentially local everywhere, cosmopolitan and impartial as the sun himself.

The mere statement of the problem is almost sufficient. The system of time must consist in simply telling *where the sun is* with respect to our terrestrial meridians—the answer in every case must be the same in all quarters of the globe. To limit the geographical knowledge necessary, insure uniformity, and afford hour-zeros, twenty-four equi-distant meridians should be agreed upon as such hour zeros, and named from some country through which, or city near which, they pass. Regard now the dial plate of the clock as the earth, the north pole at center, and meridians, twenty-four of which are actually drawn, radiating to the circumference. (Mr. Henry Farquhar suggests that the dial plate be an actual planisphere.) Bring the local meridian to the zenith and let the hour-hand, revolving once each day, point to the mean sun. The time read from such a chronometer will be the natural, or sun time, proposed in this paper. Space here forbids details with respect to the theory itself, or mention of the objections urged against its practicability; but it may be said in conclusion, in answer to an objection raised by Prof. Coffin, that the longitude of any place is given at once by the clock face at meridian transit of the mean sun, without any subtraction whatever.

Mr. HENRY FARQUHAR urged some objections to the device of reckoning time by meridians an hour apart, as not being sufficiently local to avoid a longitude correction in tables of sunrise and other astronomical events, nor sufficiently universal to escape confusion at points nearly 30 minutes from the standard meridians. He thought the need of a universal standard time, already greatly increased by railway and telegraph communication, would become still more strongly felt in the future. Inconvenience resulting from the occurrence of the 24th hour during daylight at any place, could be obviated by numbering hours beyond 24 and retaining the same day. It would not be suitable to reckon time everywhere from Greenwich midnight, since that would involve a change of day at local 10 A. M. in Sydney, (nearly noon in New Zealand) or, if the hours after 10 A. M. were counted as 25, 26, etc. of the previous day, a discrepancy in date between Australia and Europe. Hours might be reckoned from midnight at 6h. east of Greenwich, noon at 6h. west; though 5¼h. west, a meridian passing near Cumberland, Maryland, would be preferable. The longitude of a place would be the time of mean noon at that place, and count from the last-named meridian westward, from 6h. to 30h., and not from 0h. to 24h. The longitude of Washington, then, would be 23h. 53.2m., that of San Francisco, 26h. 54.6m., Honolulu, 29h. 16.4m., Auckland, 7h. 5.7m., Calcutta, 12h. 51.7m., and Greenwich, 18h. 45.0m. The 6h. meridian would pass through Bering Straits and be the line adopted for the change of date.

East of British India the day would be understood to change at 24h., which hour would arrive at some time less than 6h. after midnight. For the rest of the world, the hours would run above 24, and be diminished by 24 at the time indicated by local custom and convenience for a change of day. In Washington, for example, the conventional day might change at 36h., the hours of next day counting on from 12h., or at 39h. and count on from 15h., according as it was preferred to have the change near midnight or about 3h. after midnight. At Greenwich the hour nearest midnight would be 31h. or 7h.

Mr. Farquhar also showed a proposed form of clock-face, in which the hours were numbered from 0 to 42 in two circuits, 24 being opposite 0, and so on. Such a clock would do for all meridians, but might easily be arranged to have any desired noon-time at the top.

Mr. COFFIN remarked that he had failed to appreciate the importance of standard time to the extent to which it had been frequently advocated. If we examine the several departments, in which such time is supposed to be needed, we can better determine in what way a requirement of that kind can be best supplied.

In navigation the time of the prime meridian is a necessity; and this is furnished directly by chronometers regulated to that time, while from astronomical observations the corresponding local time may be found; and both are involved in all questions of longitude. No further standard time is needed in this department.

The use of an astronomical ephemeris also requires the time of the meridian for which it is prepared. A prime meridian common to all nations is a desideratum. But at present the maritime nations of Great Britain and the United States reckon longitudes from Greenwich, while on some of the nautical charts of Russia, Germany, and Spain, longitudes are given from Greenwich as well as from the prime meridian of each respective country. Besides this use of the meridian of Greenwich more general than of any other meridian, the meridian of 180° E. or W. from Greenwich passes near Behring Strait and through an extensive unoccupied region of the Pacific Ocean, where it will be most convenient to have the change of day, which is one less on the east side of such meridian than on the west. Indeed, the change of longitude from east to west, or the reverse, necessarily requires a change of the local day. *Where* the change is made, is arbitrary. For instance, the longitude 175° E. is equivalent to 185° W.; but October 7 in the first case is October 6 in the second. If such noting of the day, which is as much a part of the expression of the local time as are the hours and minutes, is attended to, we have the simple rule, common in navigation and the use of an ephemeris, "To the local time add the longitude if west, subtract it if east, to obtain the corresponding time of the prime meridian;" and this rule includes the day as well as its parts.

Sir John Herschel and others have proposed that longitudes should be reckoned westerly from 0 to 360°. This would complicate the expression for the local day, and congruity would require that the change of day should be at the prime meridian, which would cause great inconvenience and even confusion.

There are some observations of terrestrial phenomena, which it is desirable to have made simultaneously in the same continent or in all parts of the world. This was notably the case in the magnetic crusade some forty years ago, when certain instants of Göttingen times were specified; but the observers had no difficulty, each for himself, in determining and using his corresponding local time. And in meteorological observations, if times are prescribed in the time of any specific meridian, the observers, if of sufficient intelligence to make valuable observations, can readily convert these times into their local times, or the reverse. The constant difference of longitude, expressed in time, is all that each one requires for the purpose.

The great call for a standard time has been made with regard to railroads. A *uniform* time for each road, or connecting system of roads, is needed for regulating the times of starting and the arrival of trains, which each road can best determine for itself, and the time-tables and clocks at the several stations may be reserved for the employés of such roads only. If the time-tables published for information of the travelling public are given in the local time of each place, or a column of constants for the reduction of the published times to the local times is given, the needs of the traveller seem to be sufficiently provided for. A local time differing but little from local mean-solar time is needed to meet the wants of the social and industrial interests of the country, and if it be exactly the mean-solar time, it varies from place to place directly with the longitude.

An essential is that each time-table for railroads should state distinctly what time is used. A neglect of this has and will produce uncertainty and confusion. In a leading railroad guide I found, at a place which I visited, three time-tables for the same road, without any statement that one of them was in New York time, the others in time of other places.

The suggestion that the dials of clocks should indicate an entire day of twenty-four hours instead of a half day of twelve hours is valuable to a certain extent. This is done in astronomical clocks, and in the astronomical mode of noting time. It would be an improvement in chronometers for nautical use, but sufficient if the dial be marked into the two periods of twelve hours each, into which common, universal use divides the day.

It would seem to be impracticable to change materially the use of local-mean time, now common throughout the country; nor is such change desirable or needed.

It is only within forty years that mean time has been substituted for apparent time in many of our cities, though its advantages had long been recognized by astronomers and time regulators; and within twenty years that the sun's rising and setting have been stated in mean, instead of apparent, time in the popular almanacs of the day.

The subject-matter was further discussed by Messrs. Doolittle, Elliott, Riley, Hilgard, Gilbert, and Mussey.

Mr. G. BROWN GOODE then read a paper

ON THE FISHERIES OF THE WORLD.

This has been essentially printed in the "Cyclopædia of Political Seience, Political Economy," etc., edited by John J. Lawlor, published at Chicago, vol. 2, pp. 211–231, (Art. "Fisheries,") 1883.

222D MEETING. OCTOBER 21, 1882.

The President in the Chair.

Twenty-two members were present.

The minutes of the last meeting were read and adopted.

Mr. S. C. BUSEY read a paper

ON THE INFLUENCE OF THE CONSTANT USE OF HIGH-HEELED SHOES UPON THE HEALTH AND FORM OF THE FEMALE, AND UPON THE RELATION OF THE PELVIC ORGANS.

(The paper will appear in full in vol. 7, Gynecological Transactions.)

[Abstract.]

The foot and its coverings is not a new subject. Far more attention, however, has been given to the style and display of the covering than to the comfort and physical well-being of the foot. From this point the author gave a historical resumé of the different coverings for the feet which had been used as far back as the ancient Egyptians. The heel at first was designed to make short men look tall, and like other parts had undergone many changes to suit the whims of fashion and taste. During the reign of Louis XVI this objectionable style began to disappear, but has been again revived, and is perhaps more general now than at any previous time. Then followed a brief summary of the causes that produced deviations of form, with special reference to the effect of the constant use of French high-heeled shoes. Diagrams were exhibited showing the distortions of the feet caused by them, and the consequent changes in the joint-flexures and spinal curves. He claimed that the primary deflection took place at the base of the line of gravitation, and above this point there were greater or lesser alterations of the flexures and curves along the bony framework. Special attention was directed to the increased obliquity of the pelvis, and to the probable corresponding change in the position of the womb and other pelvic organs, which might be an important factor in the causation of some of the disorders of the female reproductive organs.

The subject-matter was discussed by various members.

A communication was submitted by Mr. THEODORE GILL

ON THE CLASSIFICATION OF THE INSECTIVOROUS MAMMALS.

In 1875 the author published a "Synopsis of Insectivorous Mammals" in the Bulletin of the United States Geological Survey of the Territories, under Hayden, (vol. 1, No. 2; 2d series, 1875, pp. 91–120,) and proposed several modifications in the classification. The principal of those modifications were (1) the union of the typical Insectivora and Dermoptera (*Galeopithecus*) is one order, as had been long before proposed by Frederic Cuvier and Wagner, but their distinction as two suborders; (2) the distribution of the true insectivores under two groups characterized by their molar dentition, and the complete subordination of the form of the body, and (3) the combination of families into super-families, and (4) the subdivision of several into subfamilies. The scheme thus promulgated has met with gratifying and unexpected favor, and has been essentially adopted by Messrs. Coues, Jordan, Dallas, Trouessart, and Dobson. Surgeon-Major Dobson's opinion is especially weighty, as he has undertaken a monograph of the order, and his opportunities for investigation have been unequalled. Since the publication of the Synopsis, in 1875, several forms have been made or become known which compel the recognition of new subordinate groups in the order; and Major Dobson has also proposed to raise the Solenodontinæ from the rank of a subfamily of Centetidæ to that of a family by the side of the latter. The assessment of the comparative value of different groups is a difficult and delicate task, and much can be said for as well as against any given proposition. The Solenodonts are doubtless as distinct from their nearest of kin as are some of the generally admitted families of mammals, and therefore it will be quite proper to recognize the family value of the type. But there are other groups of Insectivora which have been associated together in the same families which are equally or more entitled to the same distinction. Indeed, the only subfamilies of the "Synopsis of Insectivorous Mammals" which do not contrast more seem to be the Gymnurinæ and Erinaceinæ. If the Solenodontidæ are to be differentiated with family rank from the Centetidæ, so should the others. We would then have the following families:

SUBORDER DERMOPTERA.

1. Galeopithecidæ.

SUBORDER BESTIÆ.

DILAMBDODONTA.—Bestiæ with broad molar teeth surmounted by W-shaped ridges.

TUPAIOIDEA.

2. Tupaiidæ.
3. Macroscelididæ = Macroscelidinæ.
4. Rhynchocyonidæ = Rhynchocyoninæ.

ERINACEOIDEA.

5. Erinaceidæ, with the two subfamilies Gymnurinæ and Erinaceinæ.

SORICOIDEA.

6. Talpidæ = Talpinæ.
7. Myogalidæ = Myogalinæ.
8. Soricidæ.

ZALAMBDODONTA.—Bestiæ with narrow molar teeth having V-shaped ridges.

CENTETOIDA.

9. Centetidæ = Centetinæ.

10. Oryzoryctidæ = Oryzoryctinæ, *Dobson*, Mon. Insect., pp. 2, 71. 1882.

11. Solenodontidæ, *Dobson*, Mon. Insect., pp. 3, 87. 1882.

12. Potamogalidæ.

13. Geogalidæ = Geogalinæ, *Dobson*, Mon. Insect., p. 2. 1882.

CHRYSOCHLOROIDEA.

14. Chrysochloridæ.

The "Monograph of the Insectivora," by Surgeon-Major Dobson, will fill a long-felt want, and exceptionally well represent the present condition of our knowledge respecting the existing representatives of the order.

223D MEETING. NOVEMBER 4, 1882.

The President in the Chair.

Forty-five members present.

The minutes of the last meeting were read and approved.

A communication was made by Mr. G. K. GILBERT:

ON A GRAPHIC TABLE FOR COMPUTATION.

[Abstract.]

On Nov. 17th, 1881, a new method of barometric hypsometry was presented to the Society, and this has since been published in the Second Annual Report of the Geological Survey. It involves a new formula. In the application of that formula an approximate value of the required altitude is first obtained, to which a correction is then added. For the determination of this correction a table was prepared, to be entered with two arguments. Although this table was spread out on six octavo pages, and although the deduced correction is small, it was nevertheless found impracticable to avoid a double interpolation. To escape this inconvenience the graphic table was afterwards devised.

The graphic table consists of three super-imposed sets of lines. In each of two sets the lines are straight, parallel, and equidistant, and those of one set intersect those of the other at right angles.

These represent values of the two arguments. The lines of the third set are curved, and each one represents a value of the correction. In use, the straight lines representing the values of the two arguments are traced to their intersection, and from the relation of this point of intersection to the curved lines the correction is deduced.

This method is theoretically applicable to the tabulation of any quantity which is the function of two variables, but is practically useful only when the quantity to be determined is either expressible by a small number of digits, or else is subject to only a small range of variation.

A second graphic table was exhibited, having for its object the computation of altitude from horizontal distances and vertical angles as data. On this, successive values of computed altitude are indicated by parallel, equidistant, straight lines. Vertical angles are indicated by the directions of lines radiating from a point, but the intervals of these lines are not equal. Distances are measurable along these radial lines, but are not indicated in the drawing. The scale of distances is identical with that of the map, including the points whose altitudes are to be computed. The lines are drawn on tracing-linen.

For the use of this table it is postulated that the points whose altitudes are to be computed are correctly placed upon a map, and that the same map indicates a point from which the elevation or depression angles of the various points were measured. The transparent linen bearing the table is placed over the map and connected with it by a pin passing through the common origin of the radial lines, and also through the indicated position of the station from which the angles were measured. About this point as a centre the table is then moved until the radial line, indicating the vertical angle of one of the points, is brought immediately over the representation of that point upon the map, The position of that point among the parallel lines then indicates the desired altitude.

The use of this device is limited to a special case, but that case is one of frequent recurrence in the preparation of contour maps, and it is hoped that the device will lead to an economy of time.

The principle involved in the application of a *transparent* graphic table permits of the extension of the graphic table to cases involving three arguments. Two sets of lines could be drawn on a lower sheet, and two other sets on an upper transparent sheet, and these sets could be so constructed that one of them would represent a function of three variables represented by the other three.

The paper was discussed by Mr. HARKNESS and Mr. H. A. HAZEN. Mr. HARKNESS pointed out that the construction of a two-argument computation table by means of curved lines was not novel.

224TH MEETING. NOVEMBER 18, 1882.

The President in the Chair.

Forty members present.

The minutes of the last meeting were read and adopted.

Mr. E. B. ELLIOTT spoke

ON SURVIVORSHIPS, WITH TABLES AND FORMULAS OF CONSTRUCTION.

(No abstract has been furnished.)

Mr. H. A. HAZEN submitted a paper

ON THE COMING WINTER OF 1882–'83.

The following is an abstract:

It has been a great desideratum, and one which has called out the efforts of many men, to determine in advance the probable character of a season. A prominent meteorologist has inferred that the coming winter is to be a very severe one, because, as he says, "every one knows that a cold and wet summer is invariably followed by a cold and stormy winter." In order to obtain probable sequences in the weather, if we could in any way determine the mean temperature or pressure over an extensive region, it would seem as though results would be far more satisfactory than those from a single station. The following plan has been adopted for ascertaining such mean results:

We may draw isobars or any isometeorologic lines upon a map of a country; then we may rule a large number of squares upon glass or some transparent substance; and after that, by placing these squares upon the map, we may at a glance interpolate the exact pressure or temperature in each square, and a mean of all the squares would give a mean for the whole country.

Such results have been determined for the United States east of the 97th meridian for each month since July, 1873. (These were exhibited graphically before the Society.) We find a singular result on comparing these figures with similar figures for the single station of Providence, R. I., (observations at this station, from 1832 to 1876, were kindly furnished the author by the Smithsonian Institution,) namely, a striking uniformity in the values; and we may conclude that, as far as mean monthly temperatures are concerned, we may consider those at any one station fairly comparable with the same over an extensive region.

In the accompanying table each summer, and the following winter, at Providence, R. I., have been considered as cold, cool, mean, warm, or hot; and an effort has been made to establish the character of the winter that follows a summer having any one of the above characteristics:

Year.	Summer.	Winter following.	Year.	Summer.	Winter following.
1832	cold	warm	1857	cold	hot
1833	cool	warm	1858	cold	hot
1834	warm	cold	1859	mean	hot
1835	mean	cold	1860	cool	hot
1836	cold	cold	1861	cool	warm
1837	cold	mean	1862	cold	warm
1838	hot	cold	1863	cold	hot
1839	mean	cool	1864	cold	warm
1840	warm	mean	1865	mean	hot
1841	mean	hot	1866	warm	warm
1842	mean	mean	1867	mean	mean
1843	mean	mean	1868	mean	cold
1844	mean	warm	1869	cool	warm
1845	cool	cool	1870	hot	hot
1846	cold	hot	1871	mean	cold
1847	mean	hot	1872	hot	cold
1848	warm	cool	1873	mean	mean
1849	mean	hot	1874	mean	cold
1850	mean	hot	1875	cold	mean
1851	mean	cool	1876	warm	cold
1852	warm	warm	1877	warm	hot
1853	warm	cool	1878	warm	cool
1854	warm	cool	1879	mean	hot
1855	hot	cold	1880	hot	cold
1856	hot	cold	1881	warm	hot

On examining this table we find that of the eight cold summers three were followed by a hot winter, three by a warm winter, one by a mean winter, and one by a cold winter, which gives one out of eight cold summers followed by a cold winter, and six by a hot or warm winter. Taking all the cases, in forty-eight per cent. of them any summer was followed by a winter of an opposite character; in forty-two per cent. the summers or winters were mean, and in only ten per cent. of the cases were the summers followed by winters of the same character.

Making a similar comparison at Fort Snelling, Minnesota, we find, out of the sixty-eight summers and winters on record at that station, that fifty-two, or seventy-six per cent., were followed by a season of the opposite character; ten, or fifteen per cent., by a season of the same character; and six, or nine per cent., were doubtful.

We may also infer the character of the coming season for the United States by noting the movement of the permanent winter area of high pressure in respect to the Rocky mountains. It would seem as though these tended to ward off the cold if the high area settles down to the west of the range.

The winter of 1877–'78 was warm, for during every month of that season the high pressure was west of the Rockies, and the cold waves were effectually barred from the Eastern States. In December of 1877 the high pressure was spread over a vast extent of territory west of the range, and the temperature in the east rose to 7.2 degrees above the average.

The winter months of 1880–'81 were cold. During that time the high pressure was well to the east of the Rockies, and the temperature in the east fell below the average from two to six degrees. The winter of 1881–'82 was warm, as the following tabulated form shows, the plus sign indicating so many degrees above the average.

Month.	Temperature.	Position of high pressure.
1881, September	+4°.6	Normal.
October	+3°.8	Normal.
November	+2°.2	Strong west of range.
December	+7°.7	Strong west of range.
1882, January	+2°.7	Strong west of range.
February	+5°.6	Strong west of range.

It is now too early to determine exactly what the weather of the winter of 1882–'83 will be, but the indications are that it will be a medium rather than a severe one, as some have predicted. The past summer having been cold and stormy, a warm winter ought to follow; and the high pressure during last September was slightly west of the Rockies, while during October it was so far to the West and North as to rest over the Cascade range in Oregon. If it continues west of the Rocky-Mountain range a severe winter is not probable.

Mr. Henry Farquhar commenced a communication on

EXPERIMENTS IN BINARY ARITHMETIC.

The meeting was adjourned at the usual hour, (10 o'clock,) with the understanding that the unfinished communication should be taken up at a subsequent meeting.

225th Meeting. December 2, 1882.

The President in the Chair.

Fifty members present.

The minutes of the last meeting were read and adopted.

In accordance with the by-laws of the Society, the President, Mr. William B. Taylor, delivered the annual address.

ANNUAL ADDRESS

ON PHYSICS AND OCCULT QUALITIES,

BY WILLIAM B. TAYLOR.

"Vis abdita quædam."
LUCRETIUS. (*De R. N.*, lib. v. 1232.)

1. *The Dynamic and Kinematic Theories of Force.*

From the remarkable success of scientific investigation in assailing the domain of darkness,—in continually bringing the phenomena of nature more and more under the recognized empire of certain necessary laws and principles, the induction seems natural that outstanding mysteries—the ultimate constitution of matter, the nature and genesis of life and of mind itself—must in time yield to the same persistent siege of searching analysis, and be reduced to subjection under the same government, as simple servitors of an all-embracing mechanical philosophy.

In recent years, a still further induction has been ventured upon by some, to wit, that even the fundamental laws themselves of all physical action must, when properly formulated, be interpreted by simple mechanics;—all properties of matter resolved into mass or inertia, and finite extension or form,—all potentiality of matter into varying modes of motion. And it has been strongly maintained by this class of physicists, that until such consummation, the mind must still be held in thrall of mysterious unimaginable powers, the helpless devotee of "occult qualities" which science in the past has so laboriously and successfully endeavored to relegate to the shadowy limitary of metaphysics. This form of speculative doctrine, (premonitions of which may be traced back several hundred years,) may now be regarded as having attained the importance and cohesion of a school, numbering in its following a few quite eminent disciples, who agree in denying the real existence of any inherent "forces" in matter, and in holding such a designation to be merely a convenient but provisional ideal abstraction. While on the other hand the large majority of scientific thinkers (perhaps comprising most of those who have reached the conservatism of middle age) still adhere to the older conception of primeval "force" as an essential hypostasis of the operations of nature. And thus the battle so long waged (and so long practically decided) between realism and nominalism in the field of mind, bids fair to be revived (though under quite other auspices) in the field of matter. These two modes of thought may be conveniently designated the *dynamic* and the *kinematic* theories of physics. In the terminology of the *Philosophie Positive*, the dynamic theory still lingers in the shaded vale of "metaphysics," while the kinematic theory has reached the sunny hill of "positivism."* An attempt to examine and compare these divergent lines of interpretation may be a not unprofitable exercise.

The Cohesion of Matter.—Among the earliest of our experiences is the perception that the bodies around us possess in varying degrees a quality of "hardness;" and the child who gathers a rounded pebble on the beach, (if perchance inspired by its inquisitive instinct to see what the interior looks like,) discovers that to break the pebble requires the heavy and repeated strokes of a stone much larger than itself. Whence this remarkable tenacity of coherence? Whence the striking physical difference between the pebble and an equivalent mass of very fine sand?

From a large variety of facts observed in the actions of solution, of fusion, of evaporation, of the very existence of a kinetic temperature in bodies, in the phenomena of crystallization, of isomorphism, of definite and unvarying numerical mass-ratios in chemical combinations, of polymerism or serial groupings in multiple proportion, of isomerism, of allotropy, and of other more recondite habitudes of matter, the general conviction has been reached (by what has been called "a consilience of inductions") that all substance is a collection of constituent molecules of probably uniform magnitudes held together by some powerful agency. A few it is true have asserted their superiority to such popular weakness as the admission of the atomic theory; but as their vague suggestion of some continuous or colloidal form of substance has not even pretended to interpret any of the classes of phenomena just alluded to, such dis-

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*Auguste Comte, in his *Positive Philosophy*, maintains that "Forces are only motions produced or tending to be produced. - - - We hear too much still of the old metaphysical language about *forces* and the like; and it would be wise to suit our terms to our positive philosophy." (Harriet Martineau's Translation. London, 1853: book I, chap. 4.) Even *inertia* is treated as a metaphysical fiction.

sent may be summarily dismissed as the mere exhibition of an unprofitable mental captiousness.*

The kinematist repudiating any attractive force in nature would explain the strong cohesion of matter by the hypothetical external pressure of a hypothetical surrounding fluid. The Plumian professor of astronomy and physics in the University of Cambridge—James Challis—(a successor of Roger Cotes and of George B. Airy) has declared "the fundamental and only admissible idea of *force* is that of pressure, exerted either actively by the æther against the surfaces of the atoms, or as re-action of the atoms on the æther by resistance to that pressure."† And the professor of physics in the University of Edinburgh—Peter G. Tait—having also relegated the source of all material energy to the action of the highly attenuated matter diffused through space, thinks it probable that "force" has no existence, excepting as a convenient expression of a mere rate of transference of kinetic energy.‡

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* "The existence of atoms is itself an hypothesis, and *not* a probable one. - - - All dogmatic assertion upon such points is to be regarded with distrust." (*A Manual of Inorganic Chemistry*, By CHARLES W. ELIOT and FRANK H. STORER. 2d edition, revised, New York, 1868: chap. XXV, p. 605.) And yet these negative dogmatists have not shown themselves capable even of *thinking* of so elementary a fact in their science as "polymerism" apart from the terms of the atomic conception. As Prof. J. CLERK MAXWELL has well observed, "The theory that bodies apparently homogeneous and continuous are so in reality, is in its extreme form a theory incapable of development. To explain the properties of any substance by this theory is impossible." (*Encyclopædia Britannica*. 9th ed., 1875: art. "Atom," vol. III, p. 38.) The objection to atomism sometimes urged—that since magnitude is admitted abstractly or mathematically to be infinitely divisible, therefore any finite particle of matter must also be *physically* so conceived, —betrays so strange a confusion of ideas as to merit no serious answer. Yet so illustrious a mathematician and philosopher as LEONARD EULER was guilty of this gross paralogism. (*Letters to a German Princess*. May 8, 1761: vol. II, let. 9.)

† *Principles of Mathematics and Physics*. By JAMES CHALLIS. 8vo. Cambridge, 1869: hyp. V, p. 358.

‡ In an evening lecture on "Force" delivered September 8, 1876, at Glasgow, (during the session of the British Association,) Prof. TAIT announced that "there is probably no such *thing* as force at all! That it is in fact merely a convenient expression for a certain *rate*." And referring to the corpuscular hypothesis of force, he thought "The most singular thing about it is that if it be true, it will probably lead us to regard all

It is very certain, however, that the hypothetical fluid of cohesion-pressure must be something entirely different in constitution from the luminiferous æther, since any mode of action which could be imagined for compressing together the elements of matter, would necessarily be incompatible with the transmission of solar radiation having the quality and properties of the vibrations actually observed. The fantastic scheme of Le Sage (in which cohesion is effected by the quaquaversal impacts of infinitesimal corpuscles flying swiftly in all directions, and whose various sizes determine the differing collocations of chemical unions,)—notwithstanding the approval of Prof. Tait,*—scarcely requires a "serious consideration."† Nor has any form of impact, of pressure, or of undulation, yet been proffered by the ingenuity of the kinematist—either at all adequate to the maintenance of the known conditions of matter, or indeed in itself at all conformable with any known modes of action.

The dynamist having searched in vain for any plausible coordination of the indisputable facts of cohesion with an intelligible mechanical agency, simply acquiesces in the result, and without invoking the unknown or the irrelevant, accepts this established property as ultimate and inexplicable.

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kinds of energy as ultimately kinetic." (*Nature.* Sept. 21, 1876: vol. XIV, pp. 459, 463.)

The climax of kinematism however has been reached by the inventor and apostle of the "fourth state of matter,"—WILLIAM CROOKES, who is disposed to dismiss matter itself to the same limbo—of changing position: "From this point of view then matter is but *a mode of motion*; at the absolute zero of temperature the inter-molecular movement would stop, and although *something* [?] retaining the properties of inertia and weight would remain, *matter*—as we know it—would cease to exist." (*Nature.* June 17, 1880: vol. XXII, p. 153.) This seems to touch the sublime "secret" of GEORGE WILLIAM FREDERICK HEGEL, in which "nought is everything, and everything is nought."—*Seyn und Nichts ist dasselbe.*

* *Lectures on some recent advances in Physical Science.* By P. G. TAIT. 12mo. London, 1876: lect. XII, p. 299.

† "The hypothesis of Le Sage - - - is too grotesque to need serious consideration; and besides will render no account of the phenomenon of elasticity." Sir JOHN F. W. HERSCHEL, "On the Origin of Force." (*Fortnightly Review.* July 1, 1865: vol. I, p. 438. Also, *Familiar Lectures on Scientific Subjects.* 12mo. London, 1866: art. XII. pp. 466, 467.)

The Elasticity of Matter.—To select another illustration, the child throwing his rounded marble downward on a stone pavement finds te his surprise that it rebounds like his play-ball, and that he may, without stooping, catch it in his hand. What explanation is to be given of this direct and sudden reversal of movement? To this familiar quality of matter, we give the name of "elasticity." But by what more simple formula of mechanics shall we represent to ourselves this property *elasticity?* Kinematists abjuring alike objective "qualities" and subjective "abstractions" have been severely taxed in their attempts either to ignore the attribute or to reduce the phenomenon to some phase of molecular vibration.

Some few—consistent in their rejection of all quality from material substance—have boldly denied the existence of elasticity; or rather have ventured to affirm that perfectly hard or inelastic atoms or masses would on collision alike rebound, precisely as though they were elastic.* This startling conclusion—apparently necessitated by their fundamental assumption "the conservation of motion"—requires for the intelligent student of rational mechanics, no discussion.

Other kinematists have resolutely endeavored to explain the resilience of colliding bodies as the special resultant of composite motions. One of the most earnest of these has been the Italian astronomer and physicist Angelo Secchi, who in an elaborate essay on the ultimate identity of all the physical forces as simple modes of motion, remarks: "It is evident that this 'elastic force' can be admitted only as a secondary force derived from another antecedent in an aggregate of atoms, that is in a compound molecule; and that it cannot be admitted as pertaining to the elementary atoms. Indeed, elasticity in its ordinary acceptation requires a void space within the molecule to allow the form to be changed by compression and afterward restored; while on the contrary it is the necessary condition of real atoms—by conception—to be impenetrable [in-

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* This thesis was maintained by JOHN HERAPATH, in his work on *Mathematical Physics.* 8vo. 2 vols. London, 1847: (vol. I, pp. 106–137.) As stated by NEWTON however, "Bodies which are either absolutely hard, or so soft as to be void of elasticity will not rebound from one another. Impenetrability makes them only stop. If two equal bodies meet directly *in vacuo*, they will by the laws of motion stop where they meet, and lose all their motion and remain in rest, unless they be elastic and receive new motion from their spring." (*Optics.* 2d edition, 1717: book III, Qu. 31.)

compressible] and not an aggregation of other solid particles. Hence they cannot be supposed to have any internal voids in which their parts could be contracted or dilated. - - - We believe we are able to show that it is by no means a necessary position to accept this elastic property as a primitive force, but that the apparent repulsion of these atoms and their rebound originates solely from their proper motion, and for this it is sufficient simply to suppose them to be *in rotation*."* He then proceeds to develop his theory of mechanical elasticity from the co-operation of the projectile motion of bodies with the internal rotations of their constituent molecules; citing in support of his assumption, the mathematical researches of Poinsot.† In this important foundation of his system however, the zealous physicist has built upon an entirely mistaken apprehension of true mechanical principles, and hence of course upon a strange misapprehension of the actual discussion by Poinsot. This eminent mathematician who has investigated so thoroughly the theory of rotatory movements has shown that in the collision of inelastic bodies, endowed with rotation, the velocity of deflection may in special cases exceed the velocity of incidence, in other special cases may be just equal to it, and lastly in general will fall short of it, being in many cases entirely destroyed. Thus a rotating inelastic body has two points between the center of inertia and that of percussion, which on impact with a fixed resistance in the line of their direction will produce a resilience of higher velocity than that of collision,—of course by the conversion and absorption of so much of the rotary motion. There are other two points from the direction of whose impact will result a velocity just equal to that of the original motion of the body;—in the one case absorbing one-third of the rotary motion, in the other case absorbing two-thirds of it. If the impact be in the line of the center of inertia, the whole of the translatory motion is arrested without affecting the rotary motion. [In the case of two equal inelastic spheres rotating with equal and opposite velocities on parallel transverse axes and meeting at a point on their equators, the bodies

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* *L' Unitá delle Forze Fisiche;* Saggio de filosofia naturale. Del P. ANGELO SECCHI. 12mo. Rome, 1864: chap. I, sect. 6, pp. 36, 37.

† Father SECCHI's reference in a foot-note is to "*Questions dynamiques sur la percussion des corps:* pag. 21 e 29, dell' edizione a parte, ed anche il Giornale di Liouville, - - - a pag. 86."

would lose entirely their travelling motion, still retaining their rotations. So also if their axes were equally inclined so as to bring the points of impact on corresponding circles of latitude; the limiting case of which would be an impact on their poles of motion in the line of their common axes of rotation.] Lastly if a rotating inelastic body should meet a fixed resistance in the line of the center of percussion, not only the translatory—but the rotary velocity as well—would be entirely destroyed.* If we conceive a molecule as consisting of a congeries of atoms having an orbital revolution (analogous to a solar system), a very similar analysis will apply to the cases of collision.

It is very clear then that the device of storing up additional kinetic energy in the form of internal rotation (or revolution) fails utterly to reproduce the phenomena of motion exhibited by elasticity. The resulting effects cannot be admitted as at all analogous; since the internal kinetic energy assumed is either wholly or largely absorbed and exhausted by a single collision, and a second impact can never reproduce the effects of a first one; while *elastic* force remains perpetual and unimpaired by constant action.

Elasticity accordingly, equally with cohesion, is a fact of nature, a property of matter, which can neither be interpreted by any form of motion, nor resolved into any mechanical concept.† Those therefore who would formulate the elements of things devoid of

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* LOUIS POINSOT. The latter portion of a series of mathematical discussions under the general title—*Questions dynamiques sur la Percussion des Corps*; published in Liouville's *Journal de Mathematiques* for 1857: vol. II, pp. 281-308.

† "Elasticity without an action *e distanti*—even between the adjoining particles— is inconceivable. What is meant by elasticity? Surely such a constitution of the assemblage of particles as makes them recede from each other." Prof. JOHN ROBISON. (*A System of Mechanical Philosophy*. 8vo. 4 vols. Edinburgh, 1882: vol. III, p. 189.)

"An alteration of the form of a solid body is called a *strain*. In solid bodies strain is accompanied with an internal force or *stress*; those bodies in which the stress depends simply on the strain are called 'elastic,' and the property of exerting stress when strained is called elasticity. - - - The general fact that strains or changes of configuration are accompanied by stresses or internal forces, and that thereby energy is stored up in the system so strained, remains an ultimate fact which has not yet been explained as the result of any more fundamental principle." Prof. J. CLERK MAXWELL. (*Matter and Motion*. 1876: chap. V, arts. 83, 84; pp. 70, 71.

quality, have on their own declaration no right to the use of either term in considering any physical problem.

Were the examination to stop here, it might appear that the only difference between the dynamist and the kinematist is that the former—failing to find any satisfactory explanation of certain habitudes of matter, despairs of deeper insight and accordingly seeking no further, accepts the conclusion that these are insoluble; while the kinematist more hopeful, has an abiding faith that the same processes which have so successfully (or at least so largely) deciphered the riddles of light, of heat, of gaseous constitution, may be expected in time to resolve these other enigmas though they be not yet expounded. It is necessary therefore to go back still further and examine the character of this induction, by a cursory review of the postulates of the mechanical theory of light, of heat, and of the kinetics of discrete molecules.

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2. *The Theory of Molecular Kinetics.*

In the last century both light and heat were generally regarded as material emanations; the former, of radiant corpuscles, the latter, of a peculiarly rare and penetrating fluid. Earlier kinetic hypotheses of these so-called "imponderables"—however ingenious—were not supported by a sufficient induction from observed facts to justly entitle them to unqualified acceptance. And the doubts and difficulties suggested by the speculations of Newton were a striking illustration of his recognized sagacity; notwithstanding the occasional censures of modern popular lecturers, trumpeting their own superior wisdom.

The Vibratory Theory of Heat.—The fluid or "caloric" theory of heat (though often questioned or opposed) was first decisively overthrown at the close of the century by Benjamin Thompson, an expatriated American, better known as Count Rumford, whose experiments unescapably demonstrated the resolution of heat into an intestine motion, by the fact of its interminable generation in friction through the agency of continued motion.* It was not how-

* *Phil. Trans. Roy. Soc.* 1798: vol. LXXXIII, pp. 80–102. This admirable memoir read before the Royal Society of London, January 25, 1798, (in which RUMFORD—from the fact "that the source of heat generated

ever until about the middle of the present century that the conception attained a scientific definiteness and currency through the accurate determination of the kinetic or dynamic value of heat.

The Undulatory Theory of Light.—Nearly simultaneously with the work of Rumford in the field of heat, the investigations of Dr. Thomas Young, at the beginning of this century, relative especially to the interference of two luminous rays in particular cases, in like manner overthrew the theory of corpuscular emission in the field of light, by demonstrating a destruction or obliteration—quite intelligible as a conflict of wave motion, but entirely inadmissable and unthinkable as a mutual extermination of conflicting substance.* Through the refined labors of Young,—admirably assisted and re-enforced by the able efforts of his skillful and worthy rival Fresnel,—the varied and complex phenomena of dioptrics were more and more fully brought under the dominion of a rational kinetics. And thus it resulted that the new doctrine of insensible motion obtained from the scientific world a much more rapid and general acceptance in its application to light than in its application to heat. So that it was not unusual some forty or fifty

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by friction in these experiments appeared evidently to be inexhaustible," argued that this product "cannot possibly be a material substance:") may be said to furnish the first rough approximation to the mechanical equivalent of heat. The author estimated the heat produced by a one-horse power as equivalent to that obtained from the burning of nine wax candles, each three-quarters of an inch in diameter; or to the combustion of a little more than one-third of a pound of wax in two and a half hours. This essay also presents the first suggestion of the mechanical correlation of animal power with heat motion.

Dr. YOUNG held that Rumford's experiments "appear to afford an unanswerable confutation of the whole of this doctrine:—[that of a 'caloric' fluid.] - - - If heat is not a substance, it must be a quality; and this quality can only be motion." (*Lectures on Natural Philosophy.* 1807: lect. 52: vol. I, pp. 653, 654.)

"The hypothesis of caloric" says Prof. J. CLERK MAXWELL "or the theory that heat is a kind of matter is rendered untenable—first by the proof given by Rumford that heat can be *generated* at the expense of mechanical work; and secondly by the measurements of Hirn, which show that when heat does work in an engine, a portion of the heat *disappears.*" (*Theory of Heat.* 1872: chap. VIII, p. 147.)

*"*Phil. Trans. Roy. Soc.* A memoir read July 1, 1802: vol. XCII. p. 387; and one read November 24, 1803: vol. XCIV. pp. 1-16.

years ago, to find our college professors zealously inculcating the undulatory theory of light, while still maintaining the hypothesis of a "caloric" for heat.

William Herschel had found, at the beginning of the century, that the solar spectrum, as produced by an ordinary glass prism, manifested a heating power slight at the violet end, but gradually increasing to the red end, and extending a considerable distance beyond the less refrangible limit of visible rays, near which limit the maximum effect was reached.*

Johann Wilhelm Ritter, of Jena, a year later found that the chemical action of the solar spectrum, as exhibited in the darkening of silver chloride, increased toward the violet extremity, attaining a maximum beyond the most refrangible limit of luminous dispersion.† Hence, it came to be generally believed that the solar rays comprise three essentially distinct and independent kinds of energy, representing three different forms of wave-motion. This appeared the more probable from the entirely dissimilar orders of effect observed (as interpreted by the impressions of our senses), in calorific energy, in optical luminosity, and in chemical agency.

It was shown however by Alexandre Edmond Becquerel that the so-called chemical rays were not distinguishable by their refrangibility, and that photographic effects could be obtained with suitable re-agents from any region of the spectrum.‡ And finally, by the researches of Dr. John W. Draper, it was fully established that Herschel's results depended on the great distortion (as well as unequal absorption) inseparable from every prismatic or refractive spectrum, and that Ritter's results depended on a very limited and insufficient induction. And thus it has slowly come be recognized that in every normal spectrum, freed from distortion or selective absorption, (and equally freed from selective generalization), the three classes of effects, thermal, photic, and actinic, are equably or proportionally distributed; that as these several activities are equally amenable to polarization, to interference, and to spectral irradiation and absorption, there is in fact but a single form of

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**Phil. Trans. Roy. Soc.* 1800: vol. xc, pp. 291, 318, 439, 440.

† Gilbert's *Annalen der Physik.* 1801: vol. VII, p. 527. Nicholson's *Journal of Natural Philosophy,* [etc.] August, 1803: vol. V, p. 255.

‡ *Annales de Chemie et de Physique.* April, 1849: vol. XXV, pp. 447–474.

ætherial undulation, the differences of whose manifestations depend entirely upon the nature of the body, organic or inorganic, on which it falls.*

Molecular Thermo-dynamics.—Passing from the wave theory of radiation to the related subject of the internal re-actions of bodies, the application of thermo-kinetics to the facts of temperature has taught us that the molecules of all bodies are in a state of very rapid though minute movement, and that this movement, while being constantly transferred and expended, (and thus ever tending to the absolute zero,) is yet incessantly maintained in varying quantity by repeated re-enforcements from natural and artificial sources of heat, and by mutual interchanges. In the case of solid bodies, whose constituent molecules are held together by what we must call (in default of any names as yet invented by the kinematist) the qualities of *cohesion* and *adhesion*,—their mutual contact being resisted and prevented by what we must for the present call a repellant quality, the temperature motion is in the nature of an oscillation or rather irregular reverberation within the narrow limits of opposite resistances, by which the relative mean position of the particles and the stability of the body are preserved. By the term "cohesion" is designated simply the observed fact of a resistance to divellent or tensile stress; by the term "adhesion" is designated the observed fact of resistance to torsional or shearing stress.

When the energy of the molecular movements is increased until the modulus of "adhesion" is equalled, the point of melting is reached, and the molecules instead of being restored to their antecedent positions are carried irregularly from the influence of neighbor to neighbor, and thus become fluent by being deflected among each other in all possible directions. In this "liquid" condition of

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**Am. Jour. Sci.* Jan. and Feb., 1873: vol. v, pp. 25–38, and 91–98. Dr. DRAPER's results (so far as the refrangibility of radiant heat is concerned) have recently been confirmed by the refined investigations of Prof. S. P. LANGLEY, by means of his "actinic balance." (*Proceed. Am. Acad.* Jan., 1881: vol. XVI, p. 342; *Am. Jour. Sci.* March, 1881: vol. XXI, p. 187; *Nature.* Oct. 12, 1882: vol. XXVI, p. 588.)

"A ray of specified wave-length and specified plane of polarization, cannot be a combination of several different things, such as a light-ray, a heat-ray, and an actinic ray. It must be one and the same thing, which has luminous, thermal, and actinic effects." J. CLERK MAXWELL. (*Theory of Heat.* 1872: chap. XVI, p. 218.)

the mass, adjacent molecules although entirely freed from the adhesion which constitutes rigidity, yet (as has been shown by Joseph Henry) preserve their mutual cohesion practically unimpaired: * and hence devious as may be their wanderings, no portion of their excursions can be called a free path.

If the rapidity of the mean internal motion be still further accelerated until the momentum of the molecules is equal to their modulus of "cohesion," the temperature of evaporation is reached, and the molecules are impelled from their restraining bonds into a free flight, which so long as undisturbed, continues (by the first law of motion) in an indefinite straight path in the direction of impulse. The strength of these two bonds—*adhesion* and *cohesion*—differing very widely in different substances, is thus measured by the amount of kinetic energy absorbed in overcoming them,—the so-called "latent heat" of fusion and of evaporation. In the case of ice, the strength of the molecular adhesion is considerably less than the sixth part of that of the cohesion.

We thus perceive how the most solid bodies—even at low temperatures—are exposed to surface evaporation without the opportunity of passing through the liquid state; since external molecules from the great irregularity of their short oscillations, must occasionally by the composition of motions from concurrent or immediately successive shocks, acquire a velocity transcending the bonds of cohesion, and thus escape entirely from the mass.

We accordingly learn by the kinetic theory of gases that the discrete or isolated molecules are flying about in all directions in straight lines until by encounters with other molecules (or with material barriers) their course is deflected. During the brief period of encounter (the disturbance of mutual encroachment), the trajectory becomes a minute hyperbola. From the infinite variety of possible impacts we also learn that each molecule must necessarily be constantly changing within very wide limits the direction, the velocity, and the length of its free excursions;—even when a perfect equilibrium of temperature imports that the mean kinetic energy of the entire system is constant and uniform.

It is important for us to bear in mind that this wondrous theater of continual intestine commotion does not present an example of a

Proceed. Am. Phil. Soc. April 5, & May 17, 1844: vol. IV, pp. 56, 57; and 84, 85.

mechanical "perpetual motion:" the average velocity of any appreciable volume of gaseous molecules subsists only so long as no work is effected. By whatever amount any considerable number of flying particles impart motion to slower groups, or to a solid mass, by this amount do they reduce their own speed, and thus represent a diminished temperature. By whatever amount they receive any average increase of velocity from repeated impacts or from compression within a contracted inclosure, by this amount do they represent an elevation of temperature, at the expense of the bodies from which such additional energy is derived.

The Kinetic interpretation of the Laws of Gases.—It has been shown by Clausius that the number of collisions of a molecule in a given time is proportional to the mean velocity of all the molecules, to their number in a given volume, and to the square of the distance between the centers of two molecules when at nearest approach,* or at what has been called their dynamic contact. By the mathematical investigations of Krönig, Clausius, Loschmidt, and Maxwell, the foundations of a molecular physics have been successfully established; and the laws of gaseous action thus far experimentally ascertained, have been found to result deductively as the necessary consequences of the kinetic theory.

Thus the kinetic energy of any volume of molecules (which represents the temperature of the gas) being the product of molecular weight or mass by the mean square of the velocity, it follows that the relative rates of *effusion* and *diffusion* must both be inversely as the square roots of the masses,—that is of the gaseous densities;—the law of Graham.

It also follows that in the case of diffusion, by reason of the proportional retardations due to more numerous collisions from the presence of other gas, the coefficient must be lower than in the case of effusion.

In any mixture of gases, since from the mutual encounters of molecules of different mass, the average kinetic energy will be the same for all masses, or the mean squares of the velocities will be inversely as the respective masses, it follows that in different in-

*"It is to Clausius that we owe the first definite conception of the free path of a molecule and of the mean distance travelled by a molecule between successive encounters." JAMES CLERK MAXWELL. (*Encycloped. Brit.* 1875: vol. III, p. 41.)

closures at the same temperature (*i. e.*, the same energy)—for equal pressures there must be the same number of impacts on any given area, or in other words that the same volume must contain the same number of molecules whether light or heavy :—the law of Avogadro and of Ampère.

And conversely, under the same conditions of pressure (or surface impacts) and of temperature (or kinetic energy), the number of molecules being the same, and the masses of the molecules being the only variable,—the densities of different gases must be proportional to their molecular weights or the masses of their individual molecules :—the law of Gay-Lussac.

Since the sum of the moving forces or the expanding power of the molecular excursions is directly proportional to their kinetic energy, it follows that the volume of a true gas under uniform pressure must be proportional to this energy, that is to the absolute temperature :—the law of Charles and of Dalton.

Since the same kinetic energy of the molecules must exert the same impulse, or the temperatures being constant, they must have a definite mean momentum, and each molecule must execute on an average the same number of impacts with the same energy, it follows that the pressure is directly proportional to the number of molecules; or in other words that the volume of a true gas at any given temperature is inversely proportional to the pressure :—the law of Boyle and Mariotte. Or combining the last two laws, the volume of a gas multiplied by its pressure is directly proportional to the square of the mean molecular velocity, or the absolute temperature. The slight departure from the law of Boyle and Mariotte observed in most gases when compressed (the internal pressure being somewhat in defect,) indicates a small range of attraction between the molecules when brought close together.*

In addition to the external kinetic energy of the molecule due to its velocity of translation, it possesses an internal kinetic energy due to oscillation or rotation of its parts (its constituent atoms); and this internal energy according to Clausius—tends to a constant ratio with the external energy. The amount of energy received or

* "In the case of carbonic acid and other gases which are easily liquified, this deviation is very great. In all cases, however, except that of hydrogen the pressure is less than that given by Boyle's law, showing that the *virial* is on the whole due to *attractive* forces between the molecules." JAMES CLERK MAXWELL. (*Encyclopæd. Brit.* 1875: vol. III, p. 39.)

expended by a gas in gaining or losing one degree of temperature (which is known as its "specific heat") is proportional to this constant ratio; and hence the specific heat of a gas is inversely proportional to the molecular mass;—that is to say, to the specific gravity of the gas:—the law of Dulong and Petit.

As the entire kinetic energy—molecular and atomic, is necessarily tending constantly to a dynamic equilibrium both with regard to any connected volume constituting a system, and with regard to any kinetic energy of the circumambient æther as well, there is a continual and mutual transfer of such energy:—the theory of exchanges announced by Prevost.

Mean Length of Molecular Excursions.—By a neat application of the calculus of probabilities, Clausius has determined that of the whole number of free molecular excursions in a given time, (in any large inclosure,) those having less than the mean length will be 0.6321; or nearly double the number of those having the mean length or exceeding it. He supposes that under ordinary conditions, the mean length of a free excursion of our air molecules is about sixty times the mean distance between them.

Maxwell has pointed out that three phenomena dependent on the length of the free excursions of gaseous molecules, furnish functions from which the mean length of such paths may be estimated; first, the rate of gaseous diffusion (or the bodily transfer of matter); second, the rate of diffusion of their momentum, or the degree of gaseous "viscosity" (dependent on the transfer and equalization of motion); and third, the diffusion of their kinetic energy or temperature, (the conduction of heat). In our atmosphere, under ordinary conditions (30 inches and 60° F.) the mean length of the molecular path is thus estimated at about the 1 ÷ 300,000 of an inch, or about one-sixth of a wave-length of yellow light.

The average molecular velocity of oxygen has been estimated at 1640 feet per second;* and of nitrogen (which constitutes about three-fourths of our atmosphere) at 1754 feet per second; while hydrogen molecules having but one-sixteenth the weight or mass of those of oxygen, would have under the same conditions, four times their average velocity, or 6560 feet per second. And thus while a

* A velocity sufficient to carry the molecule vertically about eight miles high, if subjected to no resistance excepting gravitation.

molecule of oxygen would undergo about seven thousand million collisions in one second, a molecule of hydrogen among its fellows would undergo about seventeen thousand million collisions per second. It must be observed that the more violent the collisions of the molecules, the less is their tendency toward the cohesion of the liquid, or the adhesion of the solid form.

Probable Size of Molecules.—From various considerations it has been independently estimated by Joseph Loschmidt (1865), by G. Johnstone Stoney (1868), by William Thomson (1870), and by J. Clerk Maxwell (1873), that the effective size of the molecule is probably not smaller than the thousand-millionth of an inch, nor larger than three or four times this dimension; which is about the twenty-thousandth of a medium wave-length of light. Small as this dimension is, we may reflect that by what may be called the second power of our best microscopes, it would be easily visible,—supposing that light-waves were capable of optical efficiency at this degree of subdivision and amplification.

These estimates of molecular distances and magnitudes are of course but rough approximations; but they indicate at least the order of magnitude of very real things and agencies; and accepting them as probable, we may "compare small things with great" by saying that were the planet Venus brought within a distance from our Earth about one and a half times that of the Moon, this might represent the relative mean distance of two molecules of our atmosphere; at which separation (about fifty times their own diameters), they would probably count less than twenty million to the inch. In like manner the distance of Venus from our Earth at conjunction (as during the approaching transit of next Wednesday) would be relatively comparable to the length of a mean excursion of the molecules;—some 3,000 times their diameter. While a few of their longest free excursions would be comparable to the flight of the the same planet if carried from the Earth to beyond the orbit of Neptune.

The Relation of Molecular and Atomic Motions.—Returning again from this survey of molecular kinetics to the undulatory theory of light and heat, we may say that the true physical relation of radiation to conduction was first disclosed by the analytic spectrum,—that marvellous instrumentality which physics has presented to her daughter chemistry, as the most subtile and delicate of all her reagents. From this method of observation we have learned that each of the elements when its molecules are shocked, rings out its own peculiar series of oscillations, as if by specially adjusted tuning-forks, each responsive only to the groupings of its own established periodicities. Newton first taught us that definite refrangibility in the spectrum signifies simply definite periodicity; and he also computed the data which determine the values of these periodicities.*

The known wave-lengths of different colored light divided by their known velocity of propagation, give us the inconceivable rapidity of from 390 to 750 billions per second,† as the number of atomic impulses transmitted by the æther and appreciated by the eye. Although this compass is somewhat less than an "octave," the entire range of the visible and invisible spectrum comprises more than three octaves. This extraordinary rate of vibration, no less than its remrakable uniformity, sufficiently establishes the fact that the motions of the *molecule* ceaselessly varying in velocity, and wholly irregular in length and frequency of excursion, take no part whatever in producing ætherial undulations. It is only to the constituent parts or ultimate *atoms* of the flying molecule that the rhyth-

* NEWTON'S *Optics*. 1704: book II, part I, obs. 6. When shortly after his election to the Royal Society, Newton in a letter to the Secretary—Henry Oldenburg, (dated January 18, 1672,) proposed to offer a communication to that Society respecting his optical analysis, he spoke of it as "being the oddest if not the most considerable detection which hath hitherto been made in the operations of nature." (BIRCH'S *History of the Royal Society*. 1757: vol. III, p. 5.) Although a century and a quarter elapsed before the spectral lines were first detected by W. H. WOLLASTON, (*Phil. Trans. Roy. Soc.* June 24, 1802: vol. XCII, p. 365;) Newton was fully aware of the necessity of employing a very small hole or luminous image for obtaining a pure spectrum, and he pointed out that a narrow slit is still better; "for if this hole be an inch or two long, and but a tenth or a twentieth part of an inch broad, or narrower, the light of the image will be as simple as before, or simpler, and the image will become much broader." (*Optics*: book I, prop. IV.) For delicate observations Newton appears to have been compelled to rely on the services of an assistant; and thus he missed the consummation of his "oddest and most considerable detection of nature's operations"—the spectroscope.

† A *billion* (as is sufficiently indicated by the term itself) is the "second power of a million;" not (as is commonly taught in school-book numeration) the *third* power of a thousand, or the *second* power of an impossible number;—a surd

mic motions generating radiant light and heat must be referred We may thus picture to ourselves the monochromatic lines of the spectrum as exhibiting a second order of occult or insensible kinetics, in quality and range as different from and as much below the kinetics of the molecule, as this differs from and is below the kinetics of tangible masses.

The Origin of Atomic Motions.—With regard to the nature and origin of the atomic motions, it appears tolerably clear that they are primarily derived from the shocks of the molecules or systems of which they are the components; and that there is at every molecular collision a transfer or exchange of energy tending to equalize the internal momentum of pulsation with the external momentum of translation. The *primum mobile* is therefore the falling together of molecules under the influence either of gravitation, or of chemical affinity. While it is difficult to realize the precise manner in which molecular and atomic motions are re-distributed during the brief instants of impact, it appears in the highest degree probable that the atoms describe *elliptical orbits*, which may become circular, but never rectilinear. Were the atomic motions mere oscillations, it would appear unavoidable that under the stress of special impacts, some of them must occasionally be detached,—as in the case of molecular evaporation. But the *ultimate* molecule is unchangeable and "indivisible:"—held together in bonds incomparably stronger than those of hardest steel. And the loss of an atom may be regarded as an impossible catastrophe. Moreover, from the utter irregularity of direction in molecular encounters, obliquity of impact on the rapidly changing atoms, would appear almost a necessity: and hence would result as necessarily—elliptical paths of excursion.

In this constant play of atoms derived from repeated collisions, we must believe that these atoms are whirled in ever varying *rotations*—simultaneously with their orbital revolutions; but as these double motions form but parts of their common fund of kinetic energy, it is not probable that any special phenomena will ever distinctly reveal such axial motions;—unless indeed it be hereafter shown that *polarity* is the resultant of concerted directions of rotational or orbital axes, or of both.

The Amplitude of Atomic Orbits.—Of the actual or relative diameters of these orbits we are as ignorant as we are of the sizes of the atoms themselves. We may assume the amplitudes of the ætherial waves at their origin, to be a faithful transcript of those of the atomic excursions which generate them: and we must conclude the latter to be—even in the velocities of the highest incandescence, extremely small fractions of the length of the resulting waves. For although the amplitude of the atomic orbit represents but the square root of the brillancy, we may reflect that this latter form of energy presents an enormous range of variation. The light from Sirius—for example, supposing it to be in time twenty years in reaching us,—has but 1 ÷ 1,315,000 part of the amplitude of terrestrial sun-light; the amplitude being inversely as the distance travelled.* And there are among the visible stars doubtless some a thousand times more distant yet than Sirius.

According to the estimates of Wollaston, and of the younger Herschel, lights may vary in brilliancy forty thousand million times, representing a difference of amplitude of two hundred thousand times. To suggest some approximate idea of the form of such ætherial waves, we may liken them to earthquake waves transmitted across the surface of the ocean at the rate of six miles in a minute, which, while leaving on the tide-gage their registered amplitude of 15 inches, have for their length 150 miles: being accurately measurable waves presenting the ratio of one inch to ten miles.†

*As the bright sun Sirius is considerably larger that our sun, and probably intrinsically brighter as well, the figure 1,315,000 (representing its distance in units of sun-distance) would be somewhat reduced as a measure of relative wave-amplitude. If the intrinsic splendor of the two suns be the same, the distant one has about 64 times the surface, or eight times the diameter of our own. The probability of greater density in the former—from greater mass,—is offset by the probability of correspondingly higher temperature. Hence assuming the mean densities to be nearly the same, the gravitative pressure of equal gaseous masses on the photosphere of Sirius, would probably be in the neighborhood of eight times that upon our sun, or some 200 times that upon the surface of our earth.

† The earthquake which destroyed the city of Simoda, in Japan, in December, 1854, generated such a system of waves, which crossing the Pacific Ocean, over a distance of 4,500 miles, in the time of 12 hours and 36 minutes, left their record on the tide-gages of the Coast Survey, at San Francisco, as having a maximum amplitude of 18 inches. The height of the ocean wave at its origin was, of course, much greater than this. (*Smithsonian Report* for 1874: pp. 216, 217.—A Lecture "On Tides," by Prof. J. E. HILGARD, (at present Supt. of Coast Survey,) delivered before

Smallness of Atoms.—The extreme minuteness of the atoms is evidenced not alone by the necessary limitations of their orbital excursions under ordinary conditions, and by their inconceivable rapidity of oscillation, but even still more strikingly by the vast number of molecules which may be chemically combined and compacted within the volume of an elementary molecule,—still observing the law of Avogadro.

From such considerations we may infer that the dimensions of the ultimate atoms are probably as much below that of the composite molecule, as this is beneath a visible magnitude: or in other words, that were the molecule an object to be seen, the highest power of our best microscopes would utterly fail to detect its constituent atoms.

The Constancy of the Atomic Periods.—We have learned from the fixity of the spectral lines (whether luminous or dark) that what may be called the tones or pitches of these resonant particles are very accurately maintained through an enormous range of amplitude; that is, that the respective periods of the atomic orbits (infinitesimally brief as they appear to our slow-moving thoughts) are quite unaffected by their radii, or their rates of velocity. The evidence of these uniformities of period in descending temperatures is found in the stability of gaseous absorption lines under all degrees of cold producible; these lines remaining dark when taking up the motion of the incandescent back-ground, simply because the amplitude of the oscillation is not sufficient on the whole to impress our sense of vision. And although at very high temperatures both the number and the distinctness of the spectral lines may be considerably affected, their position (as long as visible) is not at all disturbed. That new lines should appear at increasing temperatures is not surprising, since in every case a certain width of atomic play is required to affect the eye. But that under such circumstances pre-existing lines should disappear,—as has been established by the researches of Dr. J. Plücker and Dr. J. W. Hittorf,*—so

the American Institute, Jan. 27, 1871.) It is instructive to reflect that a wave line of this order (representing an ætherial undulation)—executed by the most skillful draftsman or engraver, on any scale whatever, or with any microscopic appliances, could not be distinguished by any process of direct instrumental measurement or verification from a perfectly straight line.

**Phil. Trans. Roy. Soc.* Memoir read March 3, 1864: vol. CLV, pp. 1–29.

as to produce an entirely different spectrum, is not so easily explained. The suggestion of a disruption or disassociation of the atomic flight by centrifugal force is negatived by the fact of perfect restoration of the orbit under uniform conditions. Nor does the hypothesis of a resolution of the elementary molecules into still more elementary types, (which seems to have gained some favor,) render the physical conception of the phenomena in any respect more simple. In particular cases a precise equalization of the energies of emission, and of absorption in surrounding heated gas, might effect a neutralization and complete obliteration of one or more of the lines. And it is conceivable that a certain increase of amplitude in the ætherial wave may (as in the case of its length) cease to be recognized by the optic nerves.

The law of Atomic Orbits.—The conception being thus presented to us—of a particle moving in an elliptical or circular orbit of constant period, irrespective of the length of the radius-vector, or of the velocity, (a condition so wholly unlike the gravitative orbits of planets, observing the laws of Kepler,) what is the dynamic interpretation of such a system? This problem has been anticipated by the genius of Newton, who in his Mathematical Principles of Natural Philosophy has demonstrated the *imaginary* case,—"if the periodic times are equal, (and the velocities therefore as the radii,) the *centripetal* forces will also be as the radii." * A law of force *increasing* directly with the distance (as in the extension of an india-rubber, or of a helical steel wire spring,) is undoubtedly a very remarkable one: but whatever its range of action, it will manifestly within that range, secure the atom from all possibility of detachment.

From the perfect uniformity both of chemical and of spectroscopic indications, whether in the smallest or the largest mass of molecules,—from whatever source obtained, we are forced to conclude that the molecules of any simple gas are absolutely similar. Whether we analyze a drop of petroleum or distill an insect or a

* *Newton's Principia.* 1687: book I, sect. II, prop. 4, corol. 3. A very beautiful illustration of this orbit is presented by the conical pendulum, when the length of the suspension is very great relatively to the ranges of excursion of the ball, so that an ellipse or different circular orbits shall lie sensibly in the same plane. Another similar example is furnished by the orbits of the balls of a parabolic "governor."

plant, whether we decompose water from the Indian ocean or from Arctic snow-flake, whether we inspect with curious eye the light from sun, or star, or from remotest nebulæ at opposite confines of the heavens, we find in the spectrum of hydrogen the same fixed lines; —assuring us that these are truly the reverberations of periods incessantly repeated alike in every molecule of this particular element.* Taking this—the lightest of all known molecules, (Prout's fundamental unit of chemical equivalency,) we have within the single molecule the widely separated lines of four distinct periodicities, or atomic orbits:—the red line "C" (α) of 456 billion revolutions per second,—the greenish blue line "F" (β) of 615 billion revolutions,—the blue line near "G" (γ) of 689 billion revolutions, and the violet line "h" (δ) of 729 billion revolutions. As no form of either reciprocating or orbital movement could possibly be maintained without an equal and opposite re-action, there must necessarily exist here *at least* eight independent atoms. But it seems wholly improbable that each of these systems of motion should comprise but a single couple of atoms: and it is still more improbable that either these periods, or even the numerous additional ones disclosed in the secondary spectrum of hydrogen, represent all the atomic motions within its molecule, in view of the necessary imperfection of the optical record, and the fact that this embraces less than the third, and possibly not more than one-fourth of the whole actinic spectrum.

Physical Complexity of the Molecule.—We are therefore justified in believing that the most elementary of chemical molecules is a wonderfully complex system, comprising an unknown number of constituent units, held together by dynamic bonds whose nature we can neither guess nor conceive; and thus the atom of Newton and of Dalton has been carried downward far beyond the horizon of action at which they had imagined it—probably even to a second order of diminished magnitude.

The relations between the translatory motion of the integral gase-

* "The same kind of molecule—say that of hydrogen—has the same set of periods of vibration,—whether we procure the hydrogen from water, from coal, or from meteoric iron; and light having the same set of periods of vibration comes to us from the Sun, from Sirius, and from Arcturus." J. CLERK MAXWELL. (*Encyclopæd. Brit.* 1875: art. "Atom," vol. III, p. 48.)

ous molecule and the internal revolutions about its center of inertia present a new difficulty of conception as to the constitution and action of the ætherial medium. For while the molecule (a mere cluster of atoms) is supposed to be flying freely about without obstruction or retardation, (in order to fulfil the laws of Charles, and of Boyle and Mariotte,) the individual atoms themselves experience a very considerable resistance to their revolutions;—the precise measure of which resistance is the kinetic energy absorbed and expended by ætherial undulations. And so it results conversely, that if the motion of the æther-waves exceeds that of the molecular atoms exposed to their action, the difference of momentum is taken up by the latter, and through exchanges at molecular encounters is equalized by corresponding increments of velocity in the molecules themselves. Such is the process in all terrestrial heating by solar radiation. And this brings directly to view one important distinction between heat and light,—to wit, that while both are *radiated* in precisely the same manner, "conduction" has no existence in optical action. The only approach to any such effect in light, is found in the obscure and puzzling phenomena of fluorescence and phosphorescence, and of animal luminosity. In the case of *heat* we may have a transfer by radiation—always the result of atomic motion, by conduction—always the result of molecular motion, or by convection—always the result of mass motion.

During the time of a mean free excursion of gaseous molecules at the temperature of incandescence, the atomic periods would permit from ten to twenty thousand revolutions. But from the great amount of energy absorbed by the æther it does not appear probable that any considerable portion of such orbital movement can continue throughout the interval of a mean free path. If then it be true that in a majority of the molecular excursions the whole internal atomic motion is absorbed and destroyed, to be renewed again only by the succeeding collisions, there is a constant drain upon the molecular momentum; a condition which must alike prevail, however low may be the temperature of the gas. While there is thus a constant tendency to equalization of the orbital atomic momentum and the rectilinear molecular momentum, the total kinetic energy of the former has been estimated at not more than from two-thirds to three-fourths of the kinetic energy of the latter.

It is in the gaseous spectrum alone—that is, in the atomic motions of discrete molecules, that perfect uniformity of period, or as we may call it, perfect purity of optical tone is to be observed. With any considerable compression of a gas, that is, with any great crowding together of the molecules and shortening of their mean free excursions, whereby the increased frequency of collision is constantly disturbing the atomic orbits before their motions can be fully absorbed by the æther, there will result a momentary hastening or retarding of the normal periods, giving to the spectral lines an increased breadth or wider range of refrangibility. And when the condensation reaches that of the "liquid" or "solid" condition, preventing all free excursion, the incessant agitation of the atoms results in a universal clang or optical "noise," in which all uniformity of period seems lost, and perturbations of all possible degrees present us with the discord and confusion of a perfectly continuous spectrum.*

The Chemist has taught us that in numerous cases the normal molecule is divided into sub-molecules. Thus the relations of the compounds of arsenic, as well as of those of phosphorus, indicate the composition by half molecules of these elements; the ratios of the so-called "sesqui-salts" point to the same result; the allotropic condition of oxygen—called ozone—is formulated as having the equivalency of one and a half molecules; one molecule of aqueous vapor (and therefore of water) consists of one molecule of hydrogen and a half molecule of oxygen; two molecules of ammonia are resolved into three equal molecules of hydrogen and one of nitrogen; and a single molecule of hydrogen united with a single one of chlorine will form two molecules of hydrochloric acid,—each containing an equal division of the two constituents. Although this dichotomy of the molecule is suggestive of binary systems in some way specially linked together and at the same time susceptible of various re-arrangements, yet the fact remains that these divided molecules are still extremely complex physical systems,—apparently identical in constitution and construction, and therefore undistinguishable from each other. The Chemist however adhering too literally to the phrase of Dalton, has neglected the obvious import

*J. Clerk Maxwell has felicitously compared the atomic oscillations producing a continuous spectrum, to the clang of a bell "on which innumerable hammers are continually plying their strokes all out of time, [when] the sound will become a mere noise in which no musical note can be distinguished." (*Encyclopæd. Brit.* 1875: art. "Atom:" vol. II, p. 43.)

of the spectral lines, and speaks familiarly of the *diatomic* molecule.* It is true that the "atom" is properly a physical and not a chemical unit, since it can never be reached by any possible reactions of affinity or of decomposition. But if the term is to be still retained in chemical nomenclature, it should always be understood in its merely etymological sense of the "undivided," and not in its more popular sense of the uncompounded.

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3. *The Fallacy of Kinematic Theories.*

After this rather labored effort to approximate to some definite conception of the physical nature of the two types of invisible or elementary motion—displayed in the atomic revolutions or oscillations generating radiant undulations of the æther, and in the molecular flights and encounters generating the thermo-dynamic pressures of gaseous fluids,—let us consider what countenance these forms of motion may be supposed to lend to a kinematic theory of universal force.

It is important here to notice that by experiments on the sensible vibrations of bodies,—as of tuning-forks and pneumatic diaphragms,—translatory motions of approach and recession have been produced in light bodies. The "attractions" or "repulsions" have been shown to depend on the amplitudes of the oscillation, and the ratio of the wave-lengths to the surfaces of action; as also on the symmetrical concurrence or reversal of the phases of vibration in two confronting systems.†

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* Prof. George F. Barker in his excellent presidential address before the Chemical Section of the American Association at Buffalo, on the theme—"The Molecule and the Atom," referring to the constitution of hydrochloric acid, repeats the common view: "hence a molecule of hydrogen is composed of two atoms." (*Proceed. Am. Assoc.* August, 1876: p. 95.)

† Dr. Jules Guyot. *Des Mouvements de l'Air et des Pressions de l'Air en Mouvement.* 8vo. Paris, 1835.

Prof. Frederick Guthrie. "On Approach caused by Vibration." *L. E. D. Phil. Mag.* Nov. 1870: vol. xl, p. 354. (From his tuning-fork experiments, the author ventures the bold and startling induction: "In mechanics—in nature—there is no such thing as a pulling force.")

Prof. C. A. Bierknes of Christiania, Norway. Hydro-dynamic experi-

Irrelevancy of a Vibratory Hypothesis.—The first remark that occurs to a thoughtful student of these well-known phenomena of hydro-dynamics, (upon which narrow basis some enthusiasts have erected so wide a framework of induction,) is that between these resultant motions and any actions traceable in molecular physics,—(unless possibly in particular habitudes of electricity and magnetism,) there is not even a rough analogy. And the next and most obvious suggestion is that the absolute precedent condition of any reciprocating action whatever is the presence of the very qualities—*cohesion* and *elasticity*—for the production of which such reciprocating action is invoked. The essential powers and characteristics by which alone either atomic revolutions or molecular impacts are for an instant rendered possible, are the inherence of never-slumbering forces of attraction and repulsion. A vibratory particle (assumed by the kinematist for the avoidance of incomprehensible attributes,) is itself the most astounding—the most unrealizable in scientific thought, of all physical concepts. No atom can perform an oscillation or a revolution, or follow any other path than a straight line—excepting under the coercion of other atoms attracting and repelling. The first law of motion is that of perfect continuity both in amount and in direction. A shuttlecock rebounding in the empty air, would not be more conspicuously a dynamic solecism and impossibility than the kinematist's "vibratory particle."

Those therefore who in their backward search of causation would assign the origin of force to some incomprehensible æther action, have no more warrant from experience, induction, or reason, than those less cultured philosophers who taking "the unknown for the wonderful" habitually refer each unfamiliar phenomenon (with easy faith)—to "electricity."*

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ments on vibration. *Nature.* Aug. 18, 1881: vol. XXIV, p. 360; and Jan. 19, 1882: vol. XXV, pp. 272, 273.

Also a modification of the experiments of Prof. Bierknes, by Mr. AUGUSTUS STROH: (in air instead of in water.) *Nature.* June 8, 1882: vol. XXVI, p. 134.

* "There are not wanting those who appear very much disposed to say that the conception of *force* itself—as part and parcel of the system of the material universe—is superfluous and therefore illogical. - - - Having come to regard heat, light, electricity, as modes of motion, they seem to consider force itself as included in the same category, and think there is

Instability of a Vibratory Hypothesis.—But the kinematic embarrassment is not concluded here. Supposing the marvellous feat accomplished of effecting a rotatory resilience which should simulate in direction and amount the facts of observation, how far would such accordance justify its acceptance as the true and sufficient account of the molecular behavior, in the light of the great established principle of the conservation of energy? As a necessary corollary of this great generalization we know that every system of atomic or molecular oscillation, undulation, and impact, is directly amenable to material disturbance and to the precise mechanical equivalents of kinetic deflection, arrest, and neutralization. But as regards the fundamental qualities of atomic or molecular attractions, repulsions, and elasticities, no such disturbance, or aberration, or interference, is for an instant possible. And these fundamental qualities are persistent, and permanent, as well as unchanging. Hence the countless balls sustained in place by countless fountains, must never be permitted to decline or swerve from their required positions. Every bent spring, every loaded beam, every sustaining rope and chain and cable must therefore have expended upon it a ceaseless rain and battery of impact or of wave propulsion. Nay every solid, every liquid, must be held in its tenacious consistency by the external coercion of a never resting dynamic bombardment. In what manner is the inexhaustible supply of kinetic energy supposed to be obtained? What is its source?—and where is its escape? Why is it that the incessant and violent collisions brought into play

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'reason to believe that it depends on the diffusion of highly attenuated matter through space.'" Sir JOHN HERSCHEL. ("On the Origin of Force." *Fortnightly Review.* July 1, 1865: vol. I, p. 486. And *Familiar Lectures*, [etc.] 12mo. London, 1866: art. XII, p. 462.)

The learned physical professor in the University of Edinburgh sees "reason to believe that *force* depends upon the immediate action of highly attenuated matter diffused throughout space." (*North British Review.* February, 1864: vol. XL, p. 22,—of Am. edition. And Prof. P. G. TAIT'S *Sketch of Thermo-dynamics.* 8vo. Edinburgh, 1868: chap. I, sect. 3, p. 2.)

And the no less learned physical professor in the University of Cambridge, thinking it irrational to ascribe the occult quality of *elasticity* to any sensible molecule, finds no difficulty in relegating this property to the æther. (*L. E. D. Phil. Mag.* June, 1866: vol. XXXI, pp. 468, 469. And Prof. J. CHALLIS'S *Principles of Mathematics and Physics.* 8vo. Cambridge, 1869: pp. 316, 358, and 486.)

under this dynasty of percussion, do not speedily raise the temperature of all coherent bodies to a fierce and glowing heat?*

And this brings us face to face with the great radical—incommensurable difference between "force" and *energy,*—that the function of the former is attended with no expenditure, and is capable of no exhaustion. The truth of this bold asseveration has been tested again and again by every expedient which the most skillful and ingenious kinematists have been able to devise for its question, without the suspicion of impeachment; and it remains to-day, one of our strongest and best assured inductions.

On this broad platform rests the issue between kinematism and dynamism,—that the former inevitably contravenes and destroys that bulwark of modern physics—*the conservation of energy;* while the latter is its only support and its necessary foundation. Without the indestructible—unwasting—tensions of molecular attraction and repulsion, it lies beyond the scope of human ingenuity to devise or imagine a conservative system.

The fundamental—the inherent and incurable weakness of every attempt to supersede "force" by motion is betrayed in this,—the inadmissible supposition of a world held together only by the infinite expenditure of *work*, for whose existence no provision is devised, and for whose maintenance no motor can be suggested or conceived.†

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* Referring to the steady maintenance of material tensions by supposed ætherial motions or vortices, J. CLERK MAXWELL truly remarks: "No theory of the constitution of the ether has yet been invented which will account for such a system of molecular vortices being maintained for an indefinite time without their energy being gradually dissipated into that irregular agitation of the medium which in ordinary media is called heat." (*Encyclopædia Britannica.* 9th ed. 1878: art. "Ether:" vol. VIII, p. 572.)

† "Taking such a system in its entirety (where force exists not), there is no possibility of its reproduction. There is therefore a necessary and unceasing drain on the *vis viva* of such a system. Everything which constitutes an event, whatever its nature, exhausts some portion of the original stock. Such a system has no vitality. It feeds upon itself and has no restorative power." Sir JOHN HERSCHEL, ("On the origin of Force."—*Fortnightly Review.* July 1, 1865: vol. I, p. 487. And *Familiar Lectures,* [etc.] 1866: art. XII, p. 465.)

"It is remarkable" observes J. CLERK MAXWELL, "that of the three hypotheses which go some way toward a physical explanation of gravitation, every one involves a constant expenditure of work." (*Encyclopæd. Brit.* 9th ed. 1875: art. "Attraction:" vol. III, p. 65.)

It is the inversion of the sequence taught us by all sufficiently observant experience, that motion of any kind or form is ever the product of force, and can never be its parent.

Inadequacy of a Vibratory Hypothesis.—But after all this lavish exercise of creative power and ingenuity,—this prodigal expenditure of kinetic energy,—how surprising to find the notable invention wholly incompetent to produce the observed phenomena. Cohesive force (for example) apparently incapable of exerting any attractive power whatever beyond the range of a single layer of molecules, that is beyond the distance of perhaps the five hundred millionth of an inch from its center of action, yet exercises for an exceedingly small space within that distance a holding strength many thousands of times greater than the all-pervading power of gravitation. By what form of undulation, oscillation, or impulsion, shall we represent the tenacity of a steel wire sustaining a pull of 300,000 pounds to the square inch beyond the limits of perhaps the thousand-millionth of an inch between its molecules, yet exerting within that limit an insuperable repulsion, and again at double the distance another range of repulsion, so far resisting all human efforts, that the nicest and closest approximation of the severed ends of the wire shall fail to develop the attraction of an ounce or single grain?* By what form of partial differential equation, shall this sudden and absolute discontinuity of function be expounded? Nay rather, by what hallucination of metaphysical assumption have intelligent men been induced to waste useful time and ink and paper, on the chase of the *ignis-fatuus* of cohesive undulation or percussion?

The Authority of "Sensible" Impressions.—But it is insisted that "the principle of deriving fundamental conceptions from the indications of the senses does not admit of regarding any force varying with distance as an essential quality of matter, because according

* Prof. CHALLIS thinks "the ultimate atoms of glass are kept asunder by the repulsion of ætherial undulations which have their origin at individual atoms," and "it may be presumed that this atomic repulsion is attributable to undulations incomparably smaller than those which cause the sensation of light." (*Principles of Mathematics and Physics.* 1869: p. 456.) But the luminiferous vibrations are themselves *atomic*. What lower order of atom is then to be appealed to in support of this fanciful and inept hypothesis?

to that principle we must in seeking for the simplest idea of physical force have regard to the sense of *touch*."* Let us inquire then what is taught us by tactile experience with regard to the philosophy of physical contact. In the celebrated experiment by which Newton first measured the wave-lengths of light from the colored rings which yet bear his name, he found that on placing a piece of clean plate glass upon the convex surface of a large lens, a very considerable pressure was required to exhaust the series of outcoming interference fringes and to exhibit the central black spot. Professor Robison estimated that a pressure of at least one thousand pounds to the square inch was necessary to effect this approach to a mathematical contact between the two glasses.† And yet even with this very close and perfect physical contact it is shown that at the first appearance of the black spot between the glasses, they are still separated from actual or mathematical contact by the space of the 250,000th of an inch.

Material Contact not Absolute.—Supposing it were desired to directly communicate a push or a pull through the distance of seven miles, a perfectly straight steel bar (properly supported on friction rollers through that space) would probably be as efficient a *mechanical* means for the purpose as could well be suggested. And yet the blow of a suitably heavy hammer struck upon one of its ends would

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* Prof. JAMES CHALLIS. *Principles of Mathematics and Physics.* 1869: p. 358.

†*A System of Mechanical Philosophy.* By Prof. JOHN ROBISON: vol. I, sect. 241, p. 250. Dr. YOUNG remarks on this: "Hence it is obvious that whenever two pieces of glass strike each other without exerting a pressure equal to a thousand pounds on a square inch, they may effect each other's motion without actually coming into contact. Some persons might perhaps be disposed to attribute this repulsion to the elasticity of particles of air adhering to the glass, but I have found that the experiment succeeds equally well in the vacuum of an air-pump. We must therefore be contented to acknowledge our total ignorance of the intimate nature of forces of every kind." (*Lectures on Natural Philosophy.* 2 vols. 4to. London, 1807: lect. III: vol. I, p. 28.) And Prof. J. CLERK MAXWELL says to the same effect: "We have no evidence that real contact ever takes place between two bodies, and in fact when bodies are pressed against each other and in apparent contact, we may sometimes actually measure the distance between them, as when one piece of glass is laid on another, in which case a considerable pressure must be applied to bring the surfaces near enough

require very nearly two seconds for its transmission and delivery at the opposite end. Or if we reduce our steel punch to the more manageable length of (let us say) one foot, then the blow received by it from a hammer, and the blow given out by it at the other end, will be separated by the interval of the 18,000th part of a second. Assuming the actual approach of the hammer face to the end of the steel punch at the instant of impact to be the millionth of an inch, we may even compute the interval of time elapsing between the delivery of the blow by the hammer and its reception by the steel punch, at the 1 ÷ 216000,000000 of a second; an interval of time real enough and long enough to permit the atoms of the iron molecules to execute from 1800 to 3200 of their normal oscillations or orbital revolutions. By thus considering what is really signified by physical contact and impact, we find it to be something quite different from what the kinematist would suggest by his appeals to "the sense of touch."

The unlucky boy when struck in the face with a ball, or wounded in his finger with his jack-knife, may well refuse to be comforted by the assurance that neither the ball which bruised his face, nor the blade which penetrated and severed the capillary vessels of his finger, ever approached within the millionth of an inch of his flesh, or probably within double that distance from it. But the philosopher who aspires to construct a theory of universal force from the inductions of experience, should at least sufficiently develop his intellectual vision to avoid accepting coarse and external resemblances as evidences of co-ordinated derivation, or adopting the unanalyzed impressions of unobservant consciousness as the revelations of axiomatic truth.

Action at a Distance.—But here our investigation is undermining the very corner-stone of the kinematic system,—the repudiation of all static energy, the alleged fundamental absurdity of any mechanical action at a distance. That "a thing can no more act *where it is not* than *when* it is not," is a plain dictum of common-sense.* Even the provisional admission of such a supposition is

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to show the black spot of Newton's rings, which indicates a distance of about a ten-thousandth of a millimeter." (*Encyclopædia Britannica.* 9th ed. 1875: art. "Attraction:" vol. III, p. 63.)

* Prof. JAMES CROLL believes that "No principle will ever be generally received that stands in opposition to the old adage 'A thing cannot act

in violation of the canons of sound thought, and is contradictory of one of the most obvious aphorisms of logical metaphysics. Whatever our refinements as to the real nature of physical contact (it is said), this action is none the less a fact of constant and familiar occurrence, and is the actual method of kinetic transference manifested to our every-day observation. If we wish to give a billiard ball a definite motion in a specific direction, we do not whistle to the ball, or attempt to "psychologize" it; we strike it with a cue. Is it conceivable that "mere brute matter" should be more "spiritual" than man himself?

As these popular and taking propositions involve purely a question of physical fact, their truth can never be decided by any introspections of the consciousness, by any deductions from the "*ego cogito*," or by any disquisitions on "the theory of conception." As a question of fact, the final settlement of the nature of material *action* is to be reached only by the converging inductions of a critical *experience* (aided and enlightened by every expedient of refined investigation), and by the necessary inferences from such experience. It is very certain that a material body must exert its action—either at *some* distance, or at *no* distance, that is by absolute and perfect contact. Have we at present the means of intelligently probing this sharply defined issue?*

Action at no Distance.—It is a well-established principle, or rather fact, of dynamics that finite time is required for the production of

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where it is not.'" (*L. E. D. Phil. Mag.* December, 1867: vol. XXXIV, p. 450.) And George Henry Lewes is fully persuaded that "Action at a distance (unless understood in the sense of action through unspecified intermediates) is both logically and physically absurd." (*Problems of Life and Mind.* 1875: vol. II, appendix C, p. 484.)

* Dr. Oliver J. Lodge has remarked: "I venture to think that putting metaphysics entirely on one side we may prove in a perfectly simple and physical manner that it is impossible for two bodies *not* in contact to act directly on each other:" and he defends the position by the argument, that since action and re-action are equal and opposite, and since "work" done upon one body is equal to the "energy" so expended by the opposite body, "the distances must be equal but not opposite; that is, the two bodies must move over precisely the same distance and in the same sense: which practically asserts that they move together and are in contact so long as the action is going on." (*L. E. D. Phil. Mag.* January, 1881: vol. XI, pp. 86, 87.)

any finite velocity, or of any finite change in velocity. Only an infinite force could generate motion instantaneously, and this acting for any finite time would produce an infinite velocity. Now the impact of a moving body upon a body at rest, must occur in the absolute instant of contact. No motion could be transmitted *before* contact, for this would be the chimera—*actio in distans.* No motion could be transmitted *after* contact, for then the impinging body could evidently have no more motion than the body impinged upon. And no motion could be transmitted at the *instant* of contact, for this occupies but an infinitesimal of time. But if no motion could be communicated either before, or at, or after contact, it is very clearly established that no motion whatever could possibly be derived from impact pure and simple. This conclusion—applicable alike to an atom or a planet—remains equally unassailable whatever be the magnitudes of the bodies in action.

We are thus strongly reminded of Zeno's celebrated paradox as to the impossibility of motion. For while the kinematist very positively assures us that action at a distance is a metaphysical impossibility, the dynamist assures us no less positively that action at no distance is a demonstrated physical impossibility.* But if mere kinetic energy cannot be transferred excepting through a vacant

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* This position is so forcibly stated by Prof. JOSEPH BAYMA in his able Treatise on Molecular Physics, that a quotation from that work seems here especially appropriate. "Finite velocity cannot be communicated in an indivisible instant, as we have seen. - - - Nor can the demonstration be evaded by having recourse to the *multitude* of points among which the contact would be supposed to take place. For - - - if each individual point of matter only acquires an infinitesimal velocity (vdt), the whole multitude will acquire only an infinitesimal velocity; that is, there will be no motion caused at all. Nor can it be said that the motion is communicated by means of a *prolonged* contact. A prolonged contact is impossible unless the velocities have become equal at the very commencement of the contact. Therefore if velocity were communicated by the contact of matter with matter, it would have to be communicated in the very first instant of the contact, not in its prolongation. - - - Therefore *distance* is a necessary condition of the action of matter upon matter. Therefore the contact between the agent and the object acted upon is not material but *virtual*, inasmuch as it is by its active power (*virtus*), not by its matter, that the agent reaches the matter of the object acted upon." (*Molecular Mechanics.* 8vo. London, 1866: book I, prop. 3, pp. 14, 15.)

space, *à fortiori* must static "force" require distance as the indispensable condition of its action.

So much therefore for the vaunted dictum of "common-sense:" and so much for the antagonistic dictum whose "absurdity is so great that no man who has in philosophical matters a competent faculty of thinking can ever fall into it!"* And this absurd—this incomprehensible—this inconceivable proposition—that matter is capable of acting *only* where it is not, is proved by the incontestible conviction of reason to be a primary and necessary truth: and the wondrous scholastic dogma resisting it—supposed the sacred oracle of a mysterious intuition,—is but the detected impostor of a crude induction.

True meaning of Contact Action.—To confirm however the explicit deductions of mechanical theory by the verifications of actual experience, let us examine more closely the true character of that transmission of energy by impact which to the kinematist appears to furnish so simple and so obvious an explanation of "force." Taking the most elementary example of the *vis a tergo*, let us suppose two precisely similar billiard-balls—A and B—on the perfectly smooth surface of a frozen lake, B at rest, and A rolled toward it in the direct line joining their centers of inertia. The familiar result that A is brought to rest by the collision, and B continues the motion in the same direction prolonged, will be fluently explained by the kinematist as a mere case of conservation, or the persistence of motion,—which evidently passes at the instant of contact directly from A to B, like an electric charge.

Overlooking—first, the fallacy of a finite velocity passing into a body instantaneously (already controverted), there is a second difficulty, that *motion*—defined as a change of position in a body, or the occupation of successive portions of space by a body,—cannot exist out of the body, cannot therefore pass through the confines of the body. But admitting for the moment both these possibilities,—in the third place, how could the ball A part with *all* its motion to

* This inconsiderate utterance of NEWTON in his oft-quoted "third Bentley letter," (Feb. 25, 1693,) was wholly repudiated by him a quarter of a century later, when with a graver wisdom he asked the question: "Have not the small particles of bodies certain powers, virtues, or forces, by which they act at a distance?" (*Optics*. 2d edition. 1717: book III, query 31.) A recantation never cited by the kinematist.

another ball no larger than itself? The two possessing the same inertia, why did not *A* expend just half its motion on collision with *B*, giving the latter its equal share; and thus conserve the original momentum by the double mass moving conjointly with half the velocity? This very simple question—it is safe to affirm—can never be answered by any principles of the science of kinematics.

By the principles of dynamics, these three queries admit of a very satisfactory solution. At the moment of physical contact between the two balls, (there being still an assignable space between them,) their approaching surfaces commence mutually to encroach upon a powerful molecular repulsion crowding back and compressing more closely together vast multitudes of resisting layers of molecules on either side, until their combined pressure gradually absorbs and destroys the momentum of *A*, while simultaneously exerting an equal stress on the inertia of *B*. And thus by the necessary equality of action and re-action, the centers of inertia of the two balls pass successively through the same reversed phases of approach and recession during the brief finite interval of physical contact, attaining a relative velocity of separation precisely equal to that of the encounter: the deformations of the balls, or their compressions, being as the squares of the absorbed velocity, and their energy of recovery being as the square roots of the restored velocity. So far therefore from the original motion of *A* being transferred to *B* (as often loosely stated), it really passes continuously through every stage of decline to actual rest; and a new motion commencing from zero is gradually started in *B*, by the continued application of an elastic pressure, during a finite time.

To take one more example in illustration of the impossibility of action *at no distance*, let us suppose an ivory ball weighing one ounce to be centrally struck while at rest by another ivory ball weighing four ounces, and moving with a velocity of 10 feet per second. If we were to ignore the "occult" force of *elasticity*, and neglect the difficulties already exposed, kinematics would give the simple result of a common velocity of the two balls after impact, of 8 feet per second: 4×10 being equal to 5×8. But this is not what would happen. We should find instead that the four-ounce ball has its velocity reduced to 6 feet per second, while the one-ounce ball takes up a velocity of 16 feet per second;—just *double* that it should have taken were action at no distance a natural possibility: the latter ball absorbing (so to speak) the whole velocity and three-fifths more, while the former has expended two-fifths of its original velocity.

Here then is presented a new difficulty on the kinematic theory. In what possible manner can a body moving at a definite rate impart to another body *by simple impact* a velocity considerably higher than that possessed by itself? By kinematics, this question also must remain forever unanswered. By the established principles of dynamics—there being no actual or mathematical contact of the two balls,—the static energy of their combined compressions or repulsions acquired during the time of their physical contact precisely equals the kinetic energy of impact; and consequently on resilience refunds a precisely equal kinetic energy of separation;—to wit, a relative velocity of 10 feet per second.

Impossibility of Action at no Distance.—It turns out therefore when we examine very slightly beneath the surface of "sense information," that *impulsion* (so perfectly obvious and intelligible to the kinematist) is itself a very notable example of the ultra-sensible and recondite:*—that the vaunted philosophy of "the sense of touch" is no more able to escape from the dominion of the unseen, the hidden, the enigmatical, in causation, than is the dynamism which is held to be so superficial, credulous, and undiscerning.

And this mysterious but necessary principle of all dynamics reaches far back of the imagined cases of corporeal contact in collisions,—even to the intimate structure of the densest material;†

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*As acutely remarked by the eminent mathematician—JAMES IVORY: "A little reflection is sufficient to show that in reality we have no clearer notion of *impulse* as the cause of motion, than we have of *attraction*. We can as little give a satisfactory reason why motion should pass out of one body into another on their contact, as we can why one body should begin to move, or have its motion increased, when it is placed near another body. - - - If then we are apt to think that impulse is a clearer physical principle than attraction, there is really no good ground for the distinction; it has its origin in prejudice." (*Encyclopædia Britannica*. 8th ed. 1854: art. "Attraction:" vol IV, p. 220.)

"When the Newtonians were accused of introducing into philosophy an unknown cause which they termed *attraction*, they justly replied that they knew as much respecting attraction as their opponents did about impulse." Dr. WILLIAM WHEWELL. (*History of Scientific Ideas*. 1858: book III, chap. IX, sect. 8: vol. I, p. 278.)

† There is good reason to think that absolute contact never takes place in the component parts of the hardest and most compact solid bodies." JAMES

for it is demonstrable that the component molecules and atoms of the hardest steel are far from being in contact; that carbon molecules have room enough—even when crystal-bound in diamond—to freely execute the oscillations constituting its varying temperature by constant exchanges, and to so alter their relative excursions as to represent the changed specific gravity due to varying temperature.

The conclusion reached, we would wish to express in the most emphatic and unequivocal terms:—that in all nature we have as yet been furnished with no example of absolute contact action;—that "action at no distance" is sheer physical *impossibility;*—that in utter scorn of venerable scholastic axioms, matter is forever incapable of influencing other matter in any manner whatever or in any degree whatever—*excepting* "where it is not!" And thus the paradox of Zeno receives its solution by the thorough confutation of kinematism at every point—inductive or deductive,—theoretical or experimental.

"*Occult Qualities.*"—And now we are fully prepared to encounter the portentous arraignment of having recourse to the witch-craft of magical virtues and to the mystery of "occult qualities." What then is the precise import of this supposed obnoxious epithet *occult* as applied to material property or quality? A property whose existence is once clearly demonstrated, can scarcely with propriety be characterized as hidden, unknown, or undiscovered.* Rather are

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IVORY. (*Encyclopæd. Brit.* 8th ed: vol. IV, p. 220.) The case of simple traction by a "solid" metallic rod can be explained *only*—(as J. CLERK MAXWELL has well stated)—"by the existence of internal forces in its substance" or "between the particles of which the rod is composed, that is between bodies at distances which though small must be finite," and for these tensions acting through small distances—"we are as little able to account as for the action at any distance, however great." (*A Treatise on Electricity and Magnetism.* 8vo. 2 vols. 1873: part I, chap. V, sect. 105: vol. I, p. 123.)

* LEIBNITZ in his memorable controversy with NEWTON regarding the authorship of the infinitesimal calculus, took occasion—with a somewhat amusing though ill-tempered irrelevancy, to assail his rival's *mechanical* philosophy. In a published letter he says: "His philosophy appears to me somewhat strange, and I do not believe that it can ever be established. If all bodies possess gravity, it necessarily follows (however the defenders of the system may speak, and whatever heat they may display), that gravity

these terms applicable to pretended explanations—having no basis in fact or in reason—proffered in the vain hope of avoiding unexpected or undesired inductions. But if the phrase be designed to stigmatize either the absolute cause of original properties or their mode of operation, as obscure, hidden, inexplicable, then the epithet is but the expression of a necessary and universal truth, which may be accepted with entire satisfaction.

On contemplating the backward steps of efficient causation, we find them not only finite in number, but in any case even surprisingly few,—if we neglect the complications of perturbation, and the successions of iteration in time. When we arrive at the primitive efficient cause, (if we accept it as ultimate,) this is by admission and very definition—inexplicable; since any attempt to explain it, necessarily refers it to an antecedent cause, and thus denies it to be ultimate.* Or if this denial be insisted on, then the series of

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must be a scholastic *occult quality*, or the effect of a miracle. - - - Nor do I find a vacuum established by the reasons of Mr. Newton, or of his partizans, any more than his pretended 'universal gravitation,' or than his 'atoms.' No one—unless with very contracted views—can believe either in the vacuum, or in the atoms."

With equal dignity and cogency, NEWTON replied to this tirade, in a letter dated February 26, 1716, that he was not to be drawn by M. Leibnitz into a dispute which was nothing to the question in hand. "As for philosophy, he colludes in the significations of words, calling those things 'miracles' which create no wonder; and those things 'occult qualities' whose *causes* are occult, though the qualities themselves be manifest." (Raphson's *History of Fluxions*. Also the *Works of Isaac Newton*, edited by Samuel Horsley. 5 vols. quarto. London, 1779–1785: where both letters are given: vol. IV, pp. 596, 598.)

*Says ROGER COTES in his admirable Preface to the *Principia*: "Since causes naturally recede in a continued chain from the more compounded to the more simple, when the most simple is reached no further backward step is possible. Hence an ultimate cause cannot admit of any mechanical explanation; for if it could, it would by that very fact cease to be ultimate. Will you therefore banish ultimate causes by calling them 'occult?' Then those immediately depending on such must next alike be banished, and straightway those next following; until relieved from every vestige of a cause, philosophy shall indeed stand purged!" (Newton's *Principia*. Second edition. 1713. *Preface*.)

Says Sir WILLIAM HAMILTON, "As every effect is only produced by the concurrence of at least two causes, and as these concurrent or co-efficient causes in fact constitute the effect, it follows that the lower we descend in the series of causes, the more complex will be the product; and that the explanations is necessarily illimitable, and as necessarily beyond the grasp of human comprehension. Do what we will we cannot escape the inexorable logic of fact,—the certainty of conviction that the ultimate must in the nature of things be forever the unintelligible, the inexplicable, the inscrutable;—that (paradoxical as it may sound) no explanation can be accounted final until it has been pursued backward to the unexplainable.

And this furnishes an additional objection to the kinematic scheme,—that it leaves a vast domain—a phantasmagoria of inconsequent motions—still to be explained;—that however irrational or inexplicable its last postulate, it does not attain to that simplicity of inherent, inscrutable, attribute of power, which must ever be the test of final resolution.

He who supposes, therefore, "that the information of the senses is adequate (with the aid of mathematical reasoning) to explain phenomena of *all kinds*," who refuses to admit "that there are physical operations which are—and ever will be incomprehensible by us," betrays a very imperfect idea—no less of the impassable limitations of finite intellect, than of the fathomless profundity of nature's system.* He who thinks that by formally repudiating the mysterious, and confidently discarding the unknown, he thereby

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higher we ascend, it will be the more simple. - - - And as each step in the procedure carries us from the more complex to the more simple, and consequently nearer to unity, we at last arrive at that unity itself,—at that ultimate cause, which as ultimate cannot again be conceived as an effect." (*Lectures on Metaphysics:* lect. III, p. 42, of Am. edition. 8vo. Boston, 1859.)

Says HERBERT SPENCER, "It obviously follows that the most general truth not admitting of inclusion in any other, does not admit of interpretation. Of necessity therefore, explanation must eventually bring us down to the inexplicable. The deepest truth which we can get at must be unaccountable." (*First Principles.* 2d edition, 1869: part I, chap. 4, p. 78.)

*Prof. JAMES CHALLIS, in an essay "On the Fundamental Ideas of Matter and Force in Theoretical Physics," maintains that when there is no apparent contact between bodies, "it must still be concluded that the pressing body although invisible, exists,—unless we are prepared to admit that there are physical operations which are and ever will be incomprehensible by us. This admision is incompatible with the principles of the philosophy I am advocating, which assume that the information of the senses is adequate—with the aid of mathematical reasoning—to explain phenomena of all kinds." *L. E. D. Phil. Mag.* June, 1866: vol. XXXI, p. 467.)

abolishes or in the slightest degree diminishes his insuperable nescience of the ultimate,—but imitates the ostrich, and deludes himself.*

When men not yet emancipated from the realism of mediæval scholasticism began to turn their attention from the dreams of ontology to the actualities of sensible phenomena, it is scarcely to be wondered at that to every abstracted property of things around them, they gave "a local habitation and a name;" until the banished Nereids and Oreads, the Naiads and Dryads, the Sylphs and Gnomes, of poetic fable, were re-habilitated in a very pantheon of "occult qualities." When in a later age a larger observation and a more mathematical logic replaced these entities by more mechanical conceptions, it is perhaps as little surprising—in the momentum of re-action—that the term "occult quality" should become a shibboleth of aversion, of apprehension, and of opprobrium, the imputation of which should disturb the philosophy of even a Newton. But that we of the nineteenth century,—capable of understanding and of estimating at their approximate value the limits of these oscillations of intellectual kinetics, should be equally the timid servitors of a vocabulary—seems less excusable. Whether the intended reproach be applied to the *existence* of demonstrated qualities, or more critically to their *cause* and mode of action, is practically of little consequence. Let it be frankly avowed,—let it be boldly heralded, that in their *essence* all the primal qualities of matter *are* "occult;" and must of necessity forever remain so. Let it be recognized—with a fitting modesty—that this veil of Isis shall never be removed by mortal hands.†

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* The continental philosophers of the seventeenth century desired not only to abolish the fanciful qualities of bodies invented by their predecessors, but (as has been well said) "they tried also to abolish their own ignorance of the causes of the sensible qualities of matter. They would not have occult *causes*, and Leibnitz plainly confounds occult quality with occult cause. But it is needless to dwell upon the fact that the ultimate causes of all qualities are occult." *English Cyclopædia*—Division of *Arts and Sciences:* art. "Attraction:" vol. I, col. 739.)

† *Τὸν ἐμὸν πέπλον οὐδείς πω θνητὸς ἀπεκάλυψε.*—Inscription in the temple of Athene-Isis, at Sais on the Nile. "My veil no mortal ever withdrew."

"In bodies we see only their figures and colors, [etc.] - - - but their inward *substances* are not to be known either by our senses, or by any reflex

The Import of a "Mechanical" System.—It has been a fond assumption of the kinematist that his all-embracing system of *motion* as the origin and essence of phenomena, is pre-eminently the "mechanical" theory of nature as contrasted with a "mystical" or "transcendental" theory. It may be well therefore to consider what is really signified by the term "mechanical."

Underlying every possible conception of the simplest element of a "machine" are two essential postulates:—first, the necessity of a frame-work invested with the inherent qualities giving it structural consistence and endurance,—and secondly, the necessity of a store of potential energy by which it may be actuated and made operative: since it is an elementary truism that no machine can *originate* energy.

The geometrician who ambitious of placing his science on a more rational basis should announce a new system rejecting all assumptions and establishing its theorems by no propositions which had not first been mathematically demonstrated, might possibly receive the applause of the inexpert, but would not be likely to meet with approbation or encouragement from the great jury of his brother geometers. The physicist who proclaims that he undertakes to build up a system of mechanical laws on a foundation exclusively mechanical, acts in no sense and in no degree less irrationally. Probably his first requirement will be—"given a rigid body." But

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act of our minds." ISAAC NEWTON. (*Principia.* 1687: book III,—concluding "scholium.")

"In fact the causes of all phenomena are at last occult. There has however obtained a not unnatural presumption against such causes; and this presumption though often salutary has sometimes operated most disadvantageously to science." Sir WILLIAM HAMILTON. (*Discussions on Philosophy and Literature.* 8vo. London, 1852: appendix I, p. 611.)

"The first causes of phenomena lie beyond the limited scope of our perceptive and reasoning faculties. - - - Their intimate nature and prime origin are for us inscrutable mysteries." Dr. A. W. HOFFMAN. (*Introduction to Modern Chemistry.* 1865: lec. IX, p. 188.)

"Ultimate scientific ideas then are all representative of realities that cannot be comprehended. - - - Alike in the external and the internal worlds, the man of science sees himself in the midst of perpetual changes—of which he can discover neither the beginning nor the end. - - - In all directions his investigations eventually bring him face to face with an insoluble enigma; and he ever more clearly perceives it to be an insoluble enigma." HERBERT SPENCER. (*First Principles.* 2d ed. 1869: part I, chap. III: sect. 21, pp. 66, 67.)

by no construction, by no combination, by no involution or evolution of any purely "mechanical" process can he possibly obtain, or explain, or even conceive his postulate—a rigid body. The attempt is indeed *more* hopeless than to demonstrate an axiom by mathematical deduction. That which is the necessary basis and starting-point of any intelligible mechanics, can scarcely be supposed to be the product or derivative of such mechanics. A truly mechanical theory cannot dispense with an extraneous foundation. Those who would exclude potential causes from the field of mechanical science, but betray the hopeless—helpless nakedness and imbecility of their hypothetic fictions. "Later philosophers" says Isaac Newton, "banish the consideration of such a cause out of natural philosophy, feigning hypotheses for explaining all things *mechanically*, and referring other causes to 'metaphysics;' whereas the main business of natural philosophy is to argue from phenomena without feigning hypotheses, and to deduce causes from effects, till we come to the very first cause,—*which certainly is not mechanical.*" *

Give to the ambitious kinematic artist his cloud of sand,—or if he prefer the outfit, let him be furnished with an indefinite quantity of a perfectly continuous frictionless and incompressible fluid—bound up if you please in a chain of "vortex rings,"—by no motions or composition of motions—continued through the æons of eternity—could he ever manufacture therefrom either a lever, or a rope. The kinematic gospel of a *mechanical* theory of primeval motion is therefore a sophism and illusion. It is founded on a misconception of the very *essence* of a true mechanics. And the system that would proudly aspire to an architecture of a kosmos from the elements of matter disrobed and denuded of every quality but motion, would achieve as its highest triumph and product—a universe of dust and ashes.

Without *inertia* there could be neither transmission of motion, nor even continuity of motion. Without inertia, kinematics itself would be but an empty name. And *with* inertia, kinematics would be a science of purely rectilinear movement; for by no artifice could any other be producible. No curvature of motion—no resilience of motion—is possible without the domination and constraint of occult forces. Without "dynamics" there could be no such thing as a science of "kinetics." Without the ceaseless presence and action of occult forces there could be no such thing as the

* *Optics.* Second edition, 1717: book III, query 28.

conservation of energy; there could be no such thing as the production of energy.

Force—Real and Indispensable.—"Force" then is not a metaphorical abstraction: it is not a convenient asylum of ignorance. It is the most real,—the most fundamental,—the most inseparable of material attributes. It is the potency and faculty whereby all inorganic—no less than organic—forms are builded, and whereby alone their kaleidoscopic phenomena are revealed to our perceptions. And it is from the never resting antagonisms and reprisals of diverse forces that are made up the activity, the life, and the glory of the world in which we have our being; to whose ever changing—ever becoming—ever nascent pageantry, the poetry of antiquity has given the name—*Natura.*

In spite of every effort made to realize a favorite dream, there is no "unity of force." To the dynamics of even a single molecule, the contestation and constraint of at least two opposite resisting agencies are indispensable: and in the various play of matter, other such agencies are no less clearly manifested. Nor is the certainty of multiplicity, in the slightest degree impaired by our admitted ignorance as to the final number of primeval forces. It may be that chemical affinity, and magnetism, are like heat, and electricity,* merely derivative forms of energy; but at least this

* It is not a little remarkable that a tendency seems lately to have arisen to assign *electricity* to the station of a primitive force; and several physicists have almost simultaneously maintained its indestructibility and inconvertibility.

Dr. O. J. Lodge, in a lecture delivered at the London Institution, December 16, 1880, says: "To the question What is electricity?—We cannot assert that it is a form of matter, neither can we deny it; on the other hand we certainly cannot assert that it is a form of energy, and I should be disposed to deny it. - - - It is as impossible to generate electricity in the sense I am trying to give the word, as it is to produce matter!" (*Nature.* January 27, 1881: vol. xxiii, p. 302.)

Mr. G. Lippman, in a memoir presented to the Académie des Sciences of France, May 2, 1881, maintains that all electrical changes have an algebraic sum of zero: or in other words, that electricity can neither be created nor destroyed: the subject of the paper being "The Conservation of Electricity." (*Comptes Rendus.* 1881: vol. xcii, p. 1049.—Also, *L. E. D. Phil. Mag.* June, 1881: vol. xi, p. 474.)

Prof. Sylvanus P. Thompson, "in Elementary Lessons in Electricity," (preface,) also maintains as an important hypothesis in the treat-has not as yet been satisfactorily made out. The craving of the intellect for unity must therefore pursue its quest beyond and above the material empire of the physical forces.

The Conception of Natural "Law."—The habitudes of forces form the ultimate goal and boundary of scientific thought: and as the ascertainment and assignment of these habitudes (which we formulate as "laws" of matter) form the *object* of all science, so are their unerring certainty and uniformity of action at once the necessary *postulates* and the sole *condition* of all science. But the formulated "law" is but our mental concept of a habitude and a constancy whose method forever eludes our widest grasp, while forever challenging our most daring speculation. What *is* a law of nature? What is there behind it—to ordain or to enforce it. Do forces conform to the canons of an implicit prescription? Or is the so-called "law" but the summary and explication of autogenous deportment? Whichever be our assumption, the marvel and the incomprehensibility alike remain.

Sir John Herschel, in a playful colloquy "On Atoms," referring to their prompt obedience to the laws of their being, pithily asks: "Do they know them? Can they remember them? How else can they *obey* them?—conform to a fixed rule! Then they must be able to apply the rule as the case arises. - - - Their movements, their interchanges, their 'hates and loves,' their 'attractions and repulsions,' their 'correlations,' are all determined on the very instant. There is no hesitation, no blundering, no trial and error. A problem of dynamics which would drive Lagrange mad is solved *instanter*. A differential equation which algebraically written out would belt the earth, is integrated in an eye-twinkle." *

When we ask ourselves what these inflexible and unfailing laws of

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ment of the subject, "the conservation of electricity;" holding "that electricity, whatever it may prove to be, is not matter and is not energy," and "that it can neither be created nor destroyed." (*Nature*. May 26, 1881: vol. XXIV, p. 78.—*Elementary Lessons*, [etc.] 12 mo. London, 1881.)

The electric and caloric fluids furnish a very striking and suggestive parallelism; and the common rotatory glass cylinder would have furnished Rumford with as pertinent a theme for his argument as his gun-boring lathe.

* *Fortnightly Review*. May 15, 1865: pp. 83, 84. Also, *Familiar Lectures on Scientific Subjects*. London, 1866: pp. 456, 458.

force really mean?—Why they are thus and not otherwise?—Why they are so diverse and irreducible, and each so perfectly autocratic?—Why for example independent molecules bound in the cohesion and adhesion of the "liquid" or the "solid" condition, should exhibit an attraction for each other a thousand-fold stronger than their mutual gravitation?—Why two atoms within a molecule should cling together with a tenacity only *increasing* with their enforced centrifugal separation, while perfectly similar atoms not thus united attract each other with a strength *decreasing* with the second power of their distance?—Why the chemical affinity of dissimilar molecules shall attach them with a force incomparably greater than even that of their physical cohesion?—so that a drop of water may be shattered and lifted by the sun-beam, precipitated in snow, ground beneath a glacier, re-melted and dashed to foam in tumbling cataracts, may be combined in the solid substance of a hydrated crystal or in the complex constitution of an organic being, may be tortured in the chemist's retort or forced in hissing fury through the steam-engine, may pass through protean changes more varied than fable ever fancied, and yet in all these marvellous pilgrimages shall never loosen its structure as a compounded molecule of hydrogen and oxygen:—Why these same elements—so firmly enchained that the oxygen will quit its grasp only under the decomposing enticement of a more powerful affinity, or under the dissociative violence of a molecular velocity and clash representing the temperature of highest incandescence,—are yet so averse to separate condensation that only the combination of extremest cold and pressure attainable by human artifice has succeeded in bringing the molecules of either to a momentary liquid or solid cohesion?—we find such questionings though irresistibly suggested, as irreversibly removed outside the pale of oracle or answer. There is no mystery in the world of mind, that is not fully parallelled by mysteries as bewildering in the world of matter.

Hemmed in by the impassable limitations of a restricted experience and of a no less restricted faculty of reason, we find the finite radius of our science touching in every direction the shadowy universe of nescience; and where most we seem to know, there most we encounter the cloud-land of the unknowable. In our highest reach and proudest triumph of analytic achievement,—in that symbolical reasoning upon quantitive relation which we call *par excellence* the "mathematical,"—we find that our symbols over-step their appointed purpose, and our equations traversing the mystic region of "imaginary" expressions, transcend alike our interpretation and our comprehension.

Final Unity of Causation.—As every suggestion of an assignable limit to space or time directly impels us to "overleap all bounds," so the very definiteness of the *physical* leads us to spring in imagination beyond its frontiers, and to seek refuge in the transcendental;—not the *supernatural* as replacing or suspending the natural, but as supplementing and completing it—the ultra-natural,—in its best and highest sense the *metaphysical*. Incapable though we be of realizing in thought anything but the finite and the relative, we none the less find ourselves alike incapable of confining our thought to these; and the necessity which inexorably forbids our conception of the infinite and the absolute, no less imperiously compels our unhesitating acceptance of the unknown infinite and absolute as the unavoidable counterparts of the known finite and relative.*

Our visible material universe—to all appearance limited in extent—an islet in the boundless void,—is no less limited in duration,—at least as to any of its aspects now displayed. Nor have the falling leaf or the ageing man, the disappearance of races or the past extinction of species of genera and of orders,—more clearly inscribed upon them, the universal law and lesson of ephemeral birth development and decay, than have the starry heavens themselves. *Under the present system of dynamic law*, it is certain that as radiating and cooling bodies,

> "The stars shall fade away, the sun himself
> Grow dim with age, and nature sink in years."

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*Sir WILLIAM HAMILTON has well remarked (in his Essay on the "Philosophy of the Unconditioned"): "The *Infinite* and the *Absolute* (properly so called) are thus equally inconceivable to us. - - - We are thus taught the salutary lesson that the capacity of thought is not to be constituted into the measure of existence; and are warned from recognizing the domain of our knowledge as necessarily co-extensive with the horizon of our faith. And by a wonderful revelation we are thus in the very consciousness of our inability to conceive aught above the relative and finite, inspired with a belief in the existence of something unconditional beyond the sphere of all comprehensible reality." (*Discussions on Philosophy and Literature.* 8vo. London, 1852: part I, pp. 13 and 15.) This Essay—a Review of Victor Cousin's *Cours de Philosophie*,—was originally published in the *Edinburgh Review*, October, 1829: vol. I, pp. 194–221.

Nor is there known to science any natural process whereby this cosmic doom may be either averted, or repaired by ulterior reversal.* And when turning backward through precessive geneses of worlds and suns and systems, and recalling in imagination the heat continuously expended and dissipated during millions of millions of years, until all matter is volatilized and re-expanded in the uniform tenuity and diffusion of the primitive nebular chaos, we endeavor to extend our retrograde inspection for another billion of years,—lost in the dizzying retrospect, we find that we have neither scale, nor mechanical principle, nor hydrodynamical theory, whereby to gage or guess the antecedents of this nebular chaos.

And here again—behind the mystery and inconceivability of atomic forces, lies the still greater mystery and inconceivability of primæval nature. And yet majestic as the wondrous march of cosmic evolution—(by purely human standards), it has probably consumed no greater number of our fleeting years, than the revolutions executed by the slowest atoms in a single second of time! Or by whatever number this be multiplied, how brief an interval has it fulfilled in the great infinitude of panoramic time,—in the far-stretching ages of a past eternity.

While an intellectual necessity demands the continuity of causation and of sequence, and holds any cessation of these as positively unthinkable, we thus observe that on every side we are confronted

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* Of various suggestions (made from a teleological stand-point) for reversing the great law of "dissipation," and supplying to declining systems an *elixir vitæ* for their perpetual regeneration, perhaps the two most notable are those of Rankine and of Siemens.

William J. M. Rankine, in a paper "On the Re-concentration of the Mechanical Energy of the Universe," read before the British Association at its Belfast meeting, in September, 1852,—assuming a boundary to the ætherial medium, argues that the radiations dissipated outward, would at the limiting surface be all reflected inward to foci, at which exhausted suns would be re-kindled into incandescence, or "vaporized and resolved into their elements." (*Report Brit. Assoc.* 1852: part II,—abstracts, p. 12.—Or more fully in *L. E. D. Phil. Mag.* November, 1852: vol. IV, p. 358.)

Charles William Siemens, in a paper "On the Conservation of Solar Energy," read before the Royal Society, March 2, 1882, assuming gaseous products of combustion to be thrown off in a dissociated form from the equatorial regions of the revolving sun, (as from a centrifugal fan,) argues that they would be constantly indrawn at the polar regions, to be reburned and again given off,—in a perpetual circulation. (*Nature.* March 9, 1882: vol. XXV. pp. 440-444.)

and beset by barriers through which no loop-hole of escape appears. The mind thus baffled and bewildered in its backward inquest through illimitable series, in which to its dismay is found at no great distance—whether in atom, or in universe,—the chasm of a strange and incomprehensible discontinuity, the inevitable transition to an entirely different order of links from those made thinkable by experience, seems driven in the last resort to the unifying induction of a single, first, eternal, and all-powerful Cause—from which all other causes are dependent and derived.

This ultimate and highest induction of scientific thought—the Inscrutable made Absolute—is restful and satisfying. This ultimate and highest induction—as highest and ultimate, cannot be manipulated as a "working hypothesis." This ultimate and highest induction—as such—cannot be subjected to the subsequent verification of mathematical deduction. This ultimate and highest induction detracts nothing from the certainty of orderly sequence so irresistibly impressed upon us by every deepening channel of research, but gives us rational ground and guarantee of such unfailing regularity. This ultimate and highest induction accepting to the uttermost the mechanical interpretation of nature's administration,—whose ceaseless evolution seems ever opening up new vistas of an automatic teleology,—gives significance to our imperfect conception of a regulated system, (so necessarily involved in the very existence and operation of a "machine,") and accounts consistently for the unfaltering obedience and instantaneous response of all the countless atoms of the universe to the reign of "law," by positing behind such law—an Infinite LAW-GIVER.

In Richard Hooker's never trite though memorable words: "Of *Law* there can be no less acknowledged than that her seat is the bosom of God, her voice the harmony of the world: all things in heaven and earth do her homage,—the very least as feeling her care, and the greatest as not exempted from her power."

226TH MEETING. DECEMBER 16, 1882.

TWELFTH ANNUAL MEETING.

The President in the Chair.

About fifty members were present during the evening.

The President announced the usual order of exercises.

The minutes of the last annual meeting were read and approved.

The Secretary, Mr. GILL, read the list of members who had been elected since the last annual meeting.

The Treasurer read his report upon the finances and property of the Society. (See page 180.)

The Chairman appointed as Auditing Committee, Messrs. Thomas Antisell, Benjamin Alvord, and Otis T. Mason.

The Treasurer read the roll of names of members who were entitled to vote at the election of officers.

The Society then proceeded to ballot for the election of officers, with the following result: (See next page.)

The rough minutes of the meeting were read and approved; and the meeting then adjourned.

OFFICERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

ELECTED DECEMBER 16, 1882.

President ---------- J. W. POWELL.

Vice-Presidents ------ J. C. WELLING, J. E. HILGARD,
C. H. CRANE, J. S. BILLINGS.

Treasurer ---------- CLEVELAND ABBE.

Secretaries ---------- G. K. GILBERT, HENRY FARQUHAR.

MEMBERS AT LARGE OF THE GENERAL COMMITTEE.

W. H. DALL, C. E. DUTTON,
J. R. EASTMAN, E. B. ELLIOTT,
R. FLETCHER, WM. HARKNESS,
D. L. HUNTINGTON, GARRICK MALLERY,
C. A. SCHOTT.

STANDING COMMITTEES.

On Communications:

J. S. BILLINGS, *Chairman*. G. K. GILBERT, HENRY FARQUHAR.

On Publications:

G. K. GILBERT, *Chairman*. HENRY FARQUHAR, CLEVELAND ABBE,
S. F. BAIRD.*

*As Secretary of the Smithsonian Institution.

ANNUAL REPORT OF THE TREASURER.

WASHINGTON, D. C., *December* 17, 1881.

To the Philosophical Society of Washington:

Owing to the change in the time of presentation of the Treasurer's report, I have the honor to present herewith my annual statement as Treasurer for the years 1880 and 1881, showing a cash balance on December 16th, in the treasury, of three hundred and twenty dollars and sixteen cents, ($320.16.)

The investments of the Society consist of—

One United States bond, No. 4569 A, (registered,) of the funded loan 1891, for $1,000, yielding 4½ per cent.;

One United States bond, No. 20031, (registered,) of the funded loan of 1907, for $500, yielding 4 per cent.

The further assets of the Society consist of unpaid dues amounting to about three hundred and thirty dollars, ($330.)

The active membership of the Society is to-day about one hundred and fifty-five, (155.)

The stock on hand of the publications of the Society is about as follows, by actual count:

	No. of copies.	Price to members.
Vol. I of the Bulletin	93	$2 00
II "	92	3 00
III "	182	1 00
IV "	190	1 00
Taylor's Memoir of Prof. Henry—		
1st edition	64	50
2d "	30	1 00
Welling's Memoir of Prof. Henry	4	50

The Library has lately received, by way of exchange, about fifty volumes, but these have not yet been catalogued and arranged.

Special copies of each communication that appears in the Bulletin of the Society are promptly printed for distribution by the author; the annual volumes of the Bulletin are sent usually to about 125 domestic and foreign recipients, selected with special view to the general dissemination of information as to the activity of the Society.

The distribution of stitched annual volumes, instead of individual signatures, gives general satisfaction, and is much more economical in time and labor. Much attention is given to collecting the scattered signatures of the first volume, and thus the stock in hand of the complete volume is being slowly replenished.

Volumes I, II, and III of the Bulletin have been stereotyped and printed (with some corrections) at the expense of the Smithsonian Institution as Volume XX of the Miscellaneous Collections. It is certainly a matter of congratulation that the Society has thus assured to it the economical, permanent, and most extensive publication of its proceedings; and the general effect of this arrangement is to offer stronger inducements to our members to publish through this medium.

The expense to the Society of the publication of the first three volumes of the Bulletin was easily borne by reason of the slow accumulation of the funds in the treasury; but the cost of publication of Volume IV has been entirely defrayed out of the income of the past year, and has required very nearly the whole of our receipts, so that the balance in the treasury is now only $320.16, as compared with two hundred and fourteen dollars and eighty-two cents, ($214.82) at the beginning of 1881. The Treasurer has therefore felt himself under the necessity of distributing this volume only to members who are not in arrears.

The actual expense of the editions of 500 copies each of the respective volumes has been very nearly as follows:

Vol.	No. of signatures.	Cost per edition.	Cost per copy.
No. I	10	$386	$0 77
II	18	686	1 37
III	12	333	67
IV	12	391	78

It is therefore probable that the steady increase in the membership and work of the Society is likely soon to so increase the extent and cost of our Bulletin as to absorb our whole income.

In view of the fact that the free use of our present admirable quarters is a privilege granted by the Surgeon-General, liable at any time to be revoked, I think it important that there should always be a very considerable annual surplus to be added to the permanently-invested fund, the income of which will at some future day enable the Society to lease appropriate quarters in some central locality.

I have the honor to remain, very respectfully,

CLEVELAND ABBE, *Treasurer.*

DR. *The Philosophical Society of Washington in account with Cleveland Abbe, Treasurer.* CR.

EXPENDITURES.

Date.		Voucher.	Check.	To whom paid.	Amount.
1880.					
Jan.	29	1	36	S. N. Griffin	$8 00
Jan.	23	2	37	E. M. Whitaker	14 25
Apr.	14	3	38	Judd & Detweiler	12 50
June	29	4	39	Judd & Detweiler	43 49
July	22	5	40	C. Abbe	10 30
July	24	6	41	T. K. Collins	194 09
July	31	7	43	M. Joyce	15 50
July	28	} 8	42	Judd & Detweiler	50 00
Aug.	4		44	Judd & Detweiler	91 33
Aug.	4	9	45	C. Abbe	20 00
Aug.	13	10	46	T. K. Collins	4 00
Aug.	31	11	47	Judd & Detweiler	13 56
Dec.	6	12	48	T. K. Collins	14 20
1881.					
Jan.	6	13	49	Judd & Detweiler	52 50
					543 72
Jan.	26	----	----	To Balance on hand	214 82
				Check	$758 54

ANNUAL DUES RECEIVED DURING 1880.

Date.		1877.	1878.	1879.	1880.	1881.	Total.
1880.							
Apr.	30	------	------	$15 00	$30 00	------	$45 00
July	24	------	------	25 00	325 00	------	350 00
Aug.	25	$5 00	$5 00	10 00	20 00	------	40 00
Oct.	27	------	------	10 00	40 00	------	50 00
Nov.	9	------	------	------	70 00	$5 00	75 00
Dec.	22	------	------	10 00	20 00	15 00	45 00
Dec.	31	------	$5 00	5 00	5 00	5 00	20 00
By total dues received							$625 00
By interest on invested funds:							
One $1,000 U. S. bond, yielding 4½ per cent						$45 00	
One $500 U. S. bond, yielding 4 per cent						20 00	
Total from invested bonds							65 00
							690 00
By Balance carried forward from 1879							68 54
By Total from all sources							$758 54

CR. *The Philosophical Society of Washington in account with Cleveland Abbe, Treasurer.* CR.

EXPENDITURES.

Date.	Voucher.	Check.	To whom paid.	Amount.
1881.				
March 3	1	50	Judd & Detweiler	$29 31
March 4	2	51	Judd & Detweiler	10 00
June 6	3	52	Judd & Detweiler	10 00
Oct. 10	4	53	Judd & Detweiler	250 00
Nov. 18	4	55	Judd & Detweiler	156 20
Nov. 4	5	54	C. E. Dutton	20 65
Nov. 22	6	56	C. E. Dutton	20 00
Dec. 14	7	57	C. Abbe	5 00
Dec. 15	8	.....	F. B. Mohun	3 50
				504 66
			To Balance on hand	320 16
				$824 82

ANNUAL DUES RECEIVED DURING 1881.

Date.	1878.	1879.	1880.	1881.	1882.	Total.
Feb. 26		$5 00	$45 00	$15 00		$65 00
May 9			5 00	20 00		25 00
May 19				85 00		85 00
May 28			5 00	25 00		30 00
May 31			5 00	45 00		50 00
Sept. 30		5 00	25 00	105 00		135 00
Nov. 25				55 00		55 00
Dec. 1	$5 00	10 00	15 00	55 00		85 00
Dec. 15				15 00	$5 00	20 00
By total dues received						$550 00
By interest on invested funds:						
One $1,000 U. S. bond, 4½ per cent					$45 00	
One $500 U. S. bond, 4 per cent					15 00	
Total from invested funds						60 00
						610 00
By Balance carried forward from 1880						214 82
By total from all sources						$824 82

ANNUAL REPORT OF THE TREASURER.

WASHINGTON CITY, *Dec.* 16, 1882.

To the Philosophical Society of Washington:

I have the honor to present herewith my annual statement as Treasurer, covering the year ending with December 15, 1882, and showing a cash balance deposited with Riggs & Co. of $521.07. This balance is much larger than would have been the case had it not been decided to delay the publication of Volume V of the Bulletin.

The investment of the funds of the Society remains as in my last report, viz.:

One U. S. registered bond, $1,000, at 4½ per cent.

One U. S. registered bond, $500, at 4 per cent.

The further assets of the Society consist of unpaid annual dues to the amount of $300 for 1882, and of about $200 for 1881 and earlier years.

The number of active members is now about 150; the corresponding annual income, about 800 dollars.

The stock in hand of publications remains as about as reported by me a year ago.

An accession catalogue of the library has been recently compiled. The number of volumes at present on hand is 68; these have been presented by way of exchange; and we are especially indebted to the Royal Societies of Edinburgh, of Munich, and of New South Wales, and the Literary and Philosophical Society of Manchester for long series of volumes.

Very respectfully,

(Signed) CLEVELAND ABBE,

Treasurer.

DR. *The Philosophical Society of Washington in account with Cleveland Abbe, Treasurer, from Dec. 15, 1881, to Dec. 15, 1882.* CR.

EXPENDITURES.

Date.		Vou'r.	Check.	To whom paid.	Amount.
1881.					
Dec.	—	1	59	Judd & Detweiler	$5 40
1882.					
Jan.	30	2	60	S. J. Waldo	3 35
Jan.	18	3	61	Marcus Baker	3 50
Feb.	8	4	62	S. F. Bartlett	4 50
March	2	5	63	Marcus Baker	3 00
Feb.	28	6	64	Judd & Detweiler	21 30
June	1	7	65	Marcus Baker	6 00
May	31	8	66	Judd & Detweiler	22 95
June	15	9	67	C. Abbe, Treasurer	7 25
June	30	10	68	Judd & Detweiler	51 53
June	30	11	69	Marcus Baker	2 00
July	14	12	70	Marcus Baker	2 00
Oct.	4	13	71	Judd & Detweiler	2 50
Oct.	11	14	72	Judd & Detweiler	161 81
Oct.	14	15	73	B. F. Brown	2 00
				Total	$379 09
				Bal. on dep't with Riggs & Co.	521 07
				Total	900 16

RECEIPTS.

From what source.	Amount.	Total.
Credit by receipts as follows:		
Balance carried over from December, 1881		$320 16
Annual dues received:		
and deposited December 19, 1881	$75 00	
and deposited June 30, 1882	190 00	
and deposited July 31, 1882	175 00	
and deposited December 2, 1882	75 00	
	$515 00	
Interest on invested funds, viz.:		
One $1,000 U. S. bond, at 4½ per cent	45 00	
One $500 U. S. bond, at 4 per cent	20 00	
Total receipts		580 00
Total from all sources		$900 16

We have examined this account and find the same correct and properly vouched. December 18, 1882.

Auditors: THOMAS ANTISELL. BENJ. ALVORD. O. T. MASON.

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BULLETIN

OF THE

PHILOSOPHICAL SOCIETY

OF

WASHINGTON.

VOL. VI.

Containing the Minutes of the Society for the year 1883, and the Minutes of the Mathematical Section from its organization, March 29th, to the close of the year.

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1884.

United States of America,

Washington, D. C. January 3 1884

By ~~this post~~ Smithsonian, and on behalf of the Society, I have the honor to send you Volume ~~IV~~ VI of the Bulletin of the Philosophical Society of Washington. ~~Volumes I, II, and III were sent you in 1880.~~

Please acknowledge the receipt.

Very respectfully yours,

CLEVELAND ABBE,

Treasurer Phil. Soc. of Washington.

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1884. Mar. 1,
Gift of
The Society.

JUDD & DETWEILER, PRINTERS,
WASHINGTON, D. C.

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BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

CONSTITUTION, RULES,

LIST OF

OFFICERS AND MEMBERS,

AND

TREASURER'S REPORT.

CONSTITUTION

OF

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

ARTICLE I. The name of this Society shall be THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

ARTICLE II. The officers of the Society shall be a President, four Vice-Presidents, a Treasurer, and two Secretaries.

ARTICLE III. There shall be a General Committee, consisting of the officers of the Society and nine other members.

ARTICLE IV. The officers of the Society and the other members of the General Committee shall be elected annually by ballot; they shall hold office until their successors are elected, and shall have power to fill vacancies.

ARTICLE V. It shall be the duty of the General Committee to make rules for the government of the Society, and to transact all its business.

ARTICLE VI. This constitution shall not be amended except by a three-fourths vote of those present at an annual meeting for the election of officers, and after notice of the proposed change shall have been given in writing at a stated meeting of the Society at least four weeks previously.

STANDING RULES

FOR THE GOVERNMENT OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

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1. The Stated Meetings of the Society shall be held at 8 o'clock P. M. on every alternate Saturday; the place of meeting to be designated by the General Committee.

2. Notice of the time and place of meeting shall be sent to each member by one of the Secretaries.

When necessary, Special Meetings may be called by the President.

3. The Annual Meeting for the election of officers shall be the last stated meeting in the month of December.

The order of proceedings (which shall be announced by the Chair) shall be as follows:

First, the reading of the minutes of the last Annual Meeting.

Second, the presentation of the annual reports of the Secretaries, including the announcement of the names of members elected since the last annual meeting.

Third, the presentation of the annual report of the Treasurer.

Fourth, the announcement of the names of members who, having complied with Section 13 of the Standing Rules, are entitled to vote on the election of officers.

Fifth, the election of President.

Sixth, the election of four Vice-Presidents.

Seventh, the election of Treasurer.

Eighth, the election of two Secretaries.

Ninth, the election of nine members of the General Committee.

Tenth, the consideration of Amendments to the Constitution of the Society, if any such shall have been proposed in accordance with Article VI of the Constitution.

Eleventh, the reading of the rough minutes of the meeting.

4. Elections of officers are to be held as follows:

In each case nominations shall be made by means of an informal ballot, the result of which shall be announced by the Secretary; after which the first formal ballot shall be taken.

In the ballot for Vice-Presidents, Secretaries, and Members of the General Committee, each voter shall write on one ballot as many names as there are officers to be elected, viz., four on the first ballot for Vice-Presidents, two on the first for Secretaries, and nine on the first for Members of the General Committee; and on each subsequent ballot as many names as there are persons yet to be elected; and those persons who receive a majority of the votes cast shall be declared elected.

If in any case the informal ballot result in giving a majority for any one, it may be declared formal by a majority vote.

5. The Stated Meetings, with the exception of the annual meeting, shall be devoted to the consideration and discussion of scientific subjects.

The Stated Meeting next preceding the Annual Meeting shall be set apart for the delivery of the President's Annual Address.

6. Sections representing special branches of science may be formed by the General Committee upon the written recommendation of twenty members of the Society.*

7. Persons interested in science, who are not residents of the District of Columbia, may be present at any meeting of the Society, except the annual meeting, upon invitation of a member.

8. Similar invitations to residents of the District of Columbia, not members of the Society, must be submitted through one of the Secretaries to the General Committee for approval.

9. Invitations to attend during three months the meetings of the Society and participate in the discussion of papers, may, by a vote of nine members of the General Committee, be issued to persons nominated by two members.

10. Communications intended for publication under the auspices

* Under this rule the Mathematical Section was organized March 29, 1883. Its rules and proceedings follow the Bulletin of the General Meeting.

of the Society shall be submitted in writing to the General Committee for approval.

11.* Any paper read before a Section may be repeated, either entire or by abstract, before a general meeting of the Society, if such repetition is recommended by the General Committee of the Society.

12. New members may be proposed in writing by three members of the Society for election by the General Committee; but no person shall be admitted to the privileges of membership unless he signifies his acceptance thereof in writing within two months after notification of his election.

13. Each member shall pay annually to the Treasurer the sum of five dollars, and no member whose dues are unpaid shall vote at the annual meeting for the election of officers, or be entitled to a copy of the Bulletin.

In the absence of the Treasurer, the Secretary is authorized to receive the dues of members.

The names of those two years in arrears shall be dropped from the list of members.

Notice of resignation of membership shall be given in writing to the General Committee through the President or one of the Secretaries.

14. The fiscal year shall terminate with the Annual Meeting.

15. †Members who are absent from the District of Columbia for more than twelve months may be excused from payment of the annual assessments. They can, however, resume their membership by giving notice to the President of their wish to do so.

16. Any member not in arrears may, by the payment of one hundred dollars at any one time, become a life member, and be relieved from all further annual dues and other assessments.

All moneys received in payment of life membership shall be invested as portions of a permanent fund, which shall be directed solely to the furtherance of such special scientific work as may be ordered by the General Committee.

* Adopted, May 19, 1883. † Amended, Nov. 10, 1883.

STANDING RULES

OF THE

GENERAL COMMITTEE OF THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

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1. The President, Vice-Presidents, and Secretaries of the Society shall hold like offices in the General Committee.

2. The President shall have power to call special meetings of the Committee, and to appoint Sub-Committees.

3. The Sub-Committees shall prepare business for the General Committee, and perform such other duties as may be entrusted to them.

4. There shall be two Standing Sub-Committees; one on Communications for the Stated Meetings of the Society, and another on Publications.

5. The General Committee shall meet at half-past seven o'clock on the evening of each Stated Meeting, and by adjournment at other times.

6. For all purposes except for the amendment of the Standing Rules of the Committee or of the Society, and the election of members, six members of the Committee shall constitute a quorum.

7. The names of proposed new members recommended in conformity with Section 11 of the Standing Rules of the Society, may be presented at any meeting of the General Committee, but shall lie over for at least four weeks before final action, and the concurrence of twelve members of the Committee shall be necessary to election.

The Secretary of the General Committee shall keep a chronological register of the elections and acceptances of members.

8. These Standing Rules, and those for the government of the Society, shall be modified only with the consent of a majority of the members of the General Committee.

RULES

FOR THE

PUBLICATION OF THE BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

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1. The President's annual address shall be published in full.

2. The annual reports of the Secretaries and of the Treasurer shall be published in full.

3. When directed by the General Committee, any communication may be published in full.

4. Abstracts of papers and remarks on the same will be published, when presented to the Secretary by the author in writing within two weeks of the evening of their delivery, and approved by the Committee on Publications. Brief abstracts prepared by one of the Secretaries and approved by the Committee on Publications may also be published.

5.* If the author of any paper read before a Section of the Society desires its publication, either in full or by abstract, it shall be referred to a committee to be appointed as the Section may determine.

The report of this committee shall be forwarded to the Publication Committee by the Secretary of the Section, together with any action of the section taken thereon.

6. Communications which have been published elsewhere, so as to be generally accessible, will appear in the Bulletin by title only, but with a reference to the place of publication, if made known in season to the Committee on Publications.

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* Adopted May 19, 1883.

OFFICERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON

ELECTED DECEMBER 16, 1882.

President	J. W. POWELL.
Vice-Presidents	J. C. WELLING, J. E. HILGARD, C. H. CRANE, J. S. BILLINGS.
Treasurer	CLEVELAND ABBE.
Secretaries	G. K. GILBERT, HENRY FARQUHAR.

MEMBERS AT LARGE OF THE GENERAL COMMITTEE.

W. H. DALL, C. E. DUTTON,
J. R. EASTMAN, E. B. ELLIOTT,
R. FLETCHER, WM. HARKNESS,
D. L. HUNTINGTON, GARRICK MALLERY,*
C. A. SCHOTT.

STANDING COMMITTEES.

On Communications:

J. S. BILLINGS, *Chairman*, G. K. GILBERT, HENRY FARQUHAR.

On Publications:

G. K. GILBERT, *Chairman*, HENRY FARQUHAR, CLEVELAND ABBE, S. F. BAIRD.†

* Mr. Mallery was elected Vice-President October 13 to fill the vacancy occasioned by the death of Mr. Crane. Mr. C. V. Riley was at the same time added to the General Committee to fill its number.

† As Secretary of the Smithsonian Institution.

OFFICERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON

ELECTED DECEMBER 22, 1883.

President.............J. C. WELLING.

Vice-Presidents......J. S. BILLINGS. GARRICK MALLERY.

J. E. HILGARD. ASAPH HALL.

Treasurer..............CLEVELAND ABBE.

Secretaries.............HENRY FARQUHAR. G. K. GILBERT.

MEMBERS AT LARGE OF THE GENERAL COMMITTEE.

H. H. BATES. E. B. ELLIOTT.

W. H. DALL. ROBERT FLETCHER.

C. E. DUTTON. WILLIAM HARKNESS.

J. R. EASTMAN. J. J. KNOX.

C. V. RILEY.

STANDING COMMITTEES.

On Communications:

J. S. BILLINGS, *Chairman.* HENRY FARQUHAR. G. K. GILBERT.

On Publications:

G. K. GILBERT, *Chairman.* CLEVELAND ABBE. HENRY FARQUHAR.

S. F. BAIRD.*

* As Secretary of the Smithsonian Institution.

LIST OF MEMBERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

Corrected to December 31, 1883.

The names of founders are printed in SMALL CAPITALS.

(*d*) indicates *deceased*.

(*a*) indicates *absent* from the District of Columbia and excused from payment of dues until announcing his return.

(*r*) indicates *resigned*.

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Abbe, Cleveland	Army Signal Office. 2017 I St. N. W.	1871, Oct. 29
Abert, Sylvanus Thayer	Engineer's Office, War Department. 1724 Penn. Ave. N. W.	1875, Jan. 30
Adams, Henry	1607 H St.	1881, Feb. 5
Aldis, Asa Owen	1518 H St. N. W.	1873, Mar. 1
Allen, James	Army Signal Office. 1707 G St. N. W.	1882, Feb. 25
Alvord, Benjamin	1207 Q St. N. W.	1872, Mar. 23
ANTISELL, THOMAS	Patent Office. 1311 Q St. N. W.	1871, Mar. 13
Avery, Robert Stanton	Coast and Geodetic Survey Office. 320 A St. S. E.	1879, Oct. 11
Babcock, Orville Elias	2024 G St. N. W.	1871, June 9
Bailey, Theodorus (*d*)		1873, Mar. 1
BAIRD, SPENCER FULLERTON	Smithsonian Institution. 1445 Mass. Ave. N. W.	1871, Mar. 13
Baker, Frank	326 C St. N. W.	1881, May 14
Baker, Marcus	347 Hill St., Los Angeles, Cal.	1876, Mar. 11
Bancroft, George	1623 H St. N. W.	1875, Jan. 16
BARNES, JOSEPH K. (*d*)		1871, Mar. 13
Bates, Henry Hobart	Patent Office. The Portland	1871, Nov. 4
Beardslee, Lester Anthony (*a*)	Navy Department	1875, Feb. 27
Bell, Alexander Graham	Scott Circle, 1500 R. I. Ave.	1879, Mar. 29
Bell, Chichester Alexander	1221 Conn. Ave. N. W.	1881, Oct. 8
BENÉT, STEPHEN VINCENT	Ordnance Office, War Department. 1717 I St. N. W	1871, Mar. 13
Bessels, Emil	Smithsonian Institution. 1444 N St. N. W.	1875, Jan. 16
BILLINGS, JOHN SHAW	Surg. Genl's Office, U. S. A. 3026 N St. N. W.	1871, Mar. 13
Birney, William	456 Louisiana Ave. 1901 Harewood Ave., Le Droit Park.	1879, Mar. 29
Birnie, Rogers (*a*)	Cold Spring, Putnam Co., N. Y.	1876, Mar. 11
Bodfish, Sumner Homer	Geological Survey. 605 F St. N. W.	1883, Mar. 24
Browne, John Mills	Medical Director, U.S. N. The Portland.	1883, Nov. 24
Burchard, Horatio Chapin	Director of the Mint. Riggs House.	1879, May 10
Burgess, Edward Sanford	High School. 1214 K St. N. W.	1883, Mar. 24
Burnett, Swan Moses	1215 I St. N. W.	1879, Mar. 29

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Busey, Samuel Clagett	1525 I St. N. W	1874, Jan. 17
CAPRON, HORACE	The Portland	1871, Mar. 18
Case, Augustus Ludlow (a)	Navy Department. Bristol, R. I	1872, Nov. 16
CASEY, THOMAS LINCOLN	Lieut. Col., Corps of Engineers. 1419 K St. N. W	1871, Mar. 13
Caziarc, Louis Vasmer	Army Signal Office. 1415 G St. N.W	1882, Feb. 25
CHASE, SALMON PORTLAND (d)		1871, Mar. 13
Chamberlin, Thomas Crowder	Geological Survey	1883, Mar. 24
Chickering, John White, Jr	Deaf Mute College, Kendall Green	1874, Apr. 11
Christie, Alexander Smyth	Coast and Geodetic Survey Office. 513 6th St. N. W.	1880, Dec. 4
Clapp, William Henry (a)	1416 Corcoran St	1882, Feb. 25
Clark, Edward	Architect's Office, Capitol. 417 4th St. N. W.	1877, Feb. 24
Clark, Ezra Westcote	Revenue Marine Bureau, Treasury Department. Woodley Road.	1882, Mar. 25
Clarke, Frank Wigglesworth	Geological Survey. 1425 Q St. N.W	1874, Apr. 11
COFFIN, JOHN HUNTINGTON CRANE	1901 I St. N. W	1871, Mar. 13
Collins, Frederick (d)		1879, Oct. 21
Comstock, John Henry (a)	Cornell University, Ithaca, N. Y	1880, Feb, 14
Coues, Elliott	Smithsonian Inst. 1726 N. St. N. W	1874, Jan. 17
CRAIG, BENJAMIN FANEUIL (d)		1871, Mar. 13
Craig, Robert	Army Signal Office. 1008 I St. N. W	1873, Jan. 4
Craig, Thomas (a)	Johns Hopkins Univ., Baltimore, Md	1879, Nov. 22
CRANE, CHARLES HENRY (d)		1871, Mar. 13
Curtis, Josiah (d)		1874, Mar. 28
Cutts, Richard Dominicus (d)		1871, Apr. 20
DALL, WILLIAM HEALEY	P. O. Box 406. 1119 12th St. N. W	1871, Mar. 13
Davis, Charles Henry (d)		1874, Jan. 17
Davis, Charles Henry	Navy Department. 1705 Rhode Island Ave. N. W.	1880, June 19
Dean, Richard Crain (a)	Navy Yard, New York	1872, Apr. 23
De Caindry, William Augustin	Commissary General's Office. 924 19th St. N. W.	1881, Apr. 30
De Land, Theodore Louis	Treasury Dept. 126 7th St. N. E	1880, Dec. 18
Dewey, George (r)		1879, Feb. 15
Doolittle, Myrick Hascall	Coast and Geodetic Survey Office. 1925 I St. N. W.	1876, Feb. 12
Dorr, Fredric William (d)		1874, Jan. 17
Dunwoody, Henry Harrison Chase	Army Signal Office. 1803 G St. N. W	1873, Dec. 20
Dutton, Clarence Edward	Geological Survey. 23 Lafayette Square.	1872, Jan. 27
DYER, ALEXANDER B. (d)		1871, Mar. 13
Eastman, John Robie	Naval Observatory. 930 18th St. N.W	1871, May 27
EATON, AMOS BEEBE (d)		1871, Mar. 13
Eaton, John	Bureau of Education, Interior Dept. 712 East Capitol St.	1874, May 8
Eldredge, Stewart (a)		1871, June 9
ELLIOT, GEORGE HENRY (r)		1871, Mar. 13
ELLIOTT, EZEKIEL BROWN	Office of Government Actuary, Treasury Department. 1210 G St. N. W.	1871, Mar. 13
Emmons, Samuel Franklin	Geological Survey. 915 16th St. N.W.	1883, Apr. 7
Endlich, Frederic Miller (a)	Smithsonian Institution	1873, Mar. 1
Ewing, Charles (a)		1874, Jan. 17
Ewing, Hugh (a)	Lancaster, Ohio	1874, Jan. 17
Farquhar, Edward	Patent Office Library. 1915 H St. N.W.	1876, Feb. 12
Farquhar, Henry	Coast and Geodetic Survey Office. Brooks Station, D. C.	1881, May 14
Ferrel, William	Army Signal Office. 471 C St. N. W	1872, Nov. 16
Fletcher, Robert	Surgeon Genl's Office, U. S. A. 1326 L St. N. W.	1873, Apr. 10
Flint, Albert Stowell	Naval Observatory. 1209 Rhode Island Ave. N. W.	1882, Mar. 25
Flint, James Milton	Smithsonian Inst. Riggs House	1881, Mar. 19
FOOTE, ELISHA (d)		1871, Mar. 13
Foster, John Gray (d)		1873, Jan. 18
French, Henry Flagg (r)		1882, Mar. 25
Fristoe, Edward T.	1434 N St. N. W	1873, Mar. 20

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Gale, Leonard Dunnell (*d*)		1874, Jan. 17
Gallaudet, Edward Miner	Deaf Mute College, Kendall Green	1875, Feb. 27
Gannett, Henry	Geological Survey. 1881 Harewood Ave., Le Droit Park.	1874, Apr. 11
Gardiner, James Terry (*a*)	State Library, Albany, N. W	1874, Jan. 17
Garnett, Alexander Young P. (*r*)		1878, Mar. 16
Gihon, Albert Leary	Navy Department. 2019 Hillyer Place N. W.	1880, Dec. 18
Gilbert, Grove Karl	Geological Survey. 1424 Corcoran St.	1873, June 7
GILL, THEODORE NICHOLAS	Smithsonian Inst. 321-323 4½ St. N.W.	1871, Mar. 13
Godding, William Whitney	Government Asylum for the Insane	1879, Mar. 29
Goode, George Brown	National Museum. 1620 Mass. Ave. N. W.	1874, Jan. 31
Goodfellow, Edward	Coast and Geodetic Survey Office. 1330 19th St. N. W.	1875, Dec. 18
Goodfellow, Henry (*r*)		1871, Nov. 4
Gore, James Howard	Columbian College. 1305 Q St. N. W.	1880, Mar. 14
Graves, Edward Oziel (*a*)		1874, Apr. 11
Graves, Walter Hayden (*a*)	Denver, Colorado	1878, May 25
Greely, Adolphus Washington (*a*)		1880, June 19
Green, Bernard Richardson	1738 N St. N. W	1879, Feb. 15
Green, Francis Mathews (*a*)	Navy Department	1875, Nov. 9
GREENE, BENJAMIN FRANKLIN (*a*)	West Lebanon, N. H	1871, Mar. 13
Greene, Francis Vinton	District Commissioners' Office. 1915 G St. N. W.	1875, Apr. 10
Gunnell, Francis M	Medical Director, U. S. N. 600 20th St. N. W.	1879. Feb. 1
Hains, Peter Conover (*a*)	1824 Jefferson Place	1879, Feb. 15
HALL, ASAPH	Naval Observatory. 2715 N. St. N.W.	1871, Mar. 13
Hanscom, Isaiah (*d*)		1873, Dec. 20
HARKNESS, WILLIAM	Naval Observatory. 1415 G St. N. W.	1871, Mar. 13
Hassler, Ferdinand Augustus (*a*)	Tustin City, Los Angeles Co., Cal	1880, May 8
HAYDEN, FERDINAND VANDEVEER (*a*)	Geological Survey. 1803 Arch St., Philadelphia, Penn.	1871, Mar. 13
Hazen, Henry Allen	Army Signal Office. 1416 Corcoran St.	1882, Mar. 25
Hazen, William Babcock	Army Signal Office. 1601 K St. N.W.	1881, Feb 5
HENRY, JOSEPH (*d*)		1871, Mar. 13
Henshaw, Henry Wetherbee	Bureau of Ethnology. P. O. Box 585.	1874, Apr. 11
HILGARD, JULIUS ERASMUS	Coast and Geodetic Survey Office. 1709 Rhode Island Ave. N. W.	1871, Mar. 13
Hill, George William	Nautical Almanac Office. 314 Ind. Ave. N. W.	1879, Feb. 1
Holden, Edward Singleton (*a*)	Madison, Wisconsin	1873, June 21
Holmes, William Henry	Geological Survey. 1100 O St. N. W.	1879, Mar. 29
Hough, Franklin Benjamin (*a*)	Agricultural Department. Lowville, N. Y.	1879, Mar. 29
Howell, Edwin Eugene (*a*)	Rochester, N. Y	1874, Jan. 31
HUMPHREYS, ANDREW ATKINSON (*d*)		1871, Mar. 13
Jackson, Henry Arundel Lambe (*a*)	War Department	1875, Jan. 30
James, Owen (*a*)	Hyde Park, Penn	1880, Jan. 3
Jeffers, William Nicolson (*r*)		1877, Feb. 24
JENKINS, THORNTON ALEXANDER	2115 Penn. Ave. N. W	1871, Mar. 13
Johnson, Arnold Burges	Light House Board, Treasury Dept. 501 Maple Ave., Le Droit Park	1878, Jan. 19
Johnson, Joseph Taber	926 17th St. N. W	1879, Mar. 29
Johnston, William Waring	1603 K St. N. W	1873, Jan. 21
Kampf, Ferdinand (*d*)		1875, Dec. 18
Keith, Reuel (*a*)		1871, Oct. 29
Kerr, Washington Carruthers	Raleigh, N. C	1883, Apr. 7
Kidder, Jerome Henry	Smithsonian Institution. 1816 N St. N. W.	1880, May 8
Kilbourne, Charles Evans	Army Signal Office. Lexington House.	1880, June 19
King, Albert Freeman Africanus	726 13th St N. W	1875, Jan. 16
King, Clarence (*r*)		1879, May 10
Knox, John Jay	Treasury Dept. 1127 10th St. N. W.	1874, May 8
Kummell, Charles Hugo	Coast and Geodetic Survey Office. 608 Q St. N. W.	1882, Mar. 25
LANE, JONATHAN HOMER (*d*)		1871, Mar. 13
Lawver, Winfield Peter	Mint Bureau, Treasury Department. 1912 I St. N. W.	1881, Feb. 19

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Lee, William	2111 Penn. Ave. N. W.	1874, Jan. 17
Lefavour, Edward Brown	Coast and Geodetic Survey Office. 117 C St. S. E.	1882, Dec. 16
Lincoln, Nathan Smith	1514 H St. N. W	1871, May 27
Lockwood, Henry H. (r)		1871, Oct. 29
Loomis, Eben Jenks	Nautical Almanac Office. 1413 College Hill Terrace N. W.	1880, Feb. 14
Lull, Edward Phelps	Navy Department	1875, Dec. 4
Lyford, Stephen Carr (r)		1873, Jan. 18
MacCauley, Henry Clay (a)	Helena, Montana	1880, Jan. 3
McGee, W. J.	Geological Survey. 512 13th St. N. W.	1883, Nov. 10
McGuire, Frederick Bauders	1306 F St. N. W. 614 E St. N. W	1879, Feb. 15
Mack, Oscar A. (d)		1872, Jan. 27
McMurtrie, William (a)	Champaign, Ill	1876, Feb. 26
Mallery, Garrick	Bureau of Ethnology. P. O. Box 585. 1323 N St. N. W.	1875, Jan. 30
Marvin, Joseph Badger (a)		1878, May 25
Marvine, Archibald Robertson (d)		1874, Jan. 31
Mason, Otis Tufton	Columbian College. 1305 Q St. N.W	1875, Jan. 30
MEEK, FIELDING BRADFORD (d)		1871, Mar. 13
Meigs, Montgomery (a)	War Department. Rock Island, Ill	1877, Mar. 24
MEIGS, MONTGOMERY CUNNINGHAM	1239 Vermont Ave. N. W	1871, Mar. 13
Milner, James William (d)		1874, Jan. 31
Morgan, Ethelbert Carroll	918 E St. N. W	1883, Oct. 13
Morris, Martin Ferdinand (a)		1877, Feb. 24
Mussey, Reuben Delavan	P. O. Box 618. 508 5th St. N. W	1881, Dec. 3
MYER, ALBERT J. (d)		1871, Mar. 13
Myers, William (a)	War Department	1871, June 23
NEWCOMB, SIMON	Navy Department. Stoddart Street	1871, Mar. 13
Nichols, Charles Henry (a)		1872, May 4
NICHOLSON, WALTER LAMB	1322 I St. N. W	1871, Mar. 13
Nordhoff, Charles	Alpine, Bergen Co., N. J.	1879, May 10
Osborne, John Walter	212 Delaware Ave. N. E.	1878, Dec. 7
OTIS, GEORGE ALEXANDER (d)		1871, Mar. 13
PARKE, JOHN GRUBB	Engineer Bureau, War Department. 16 Lafayette Square.	1871, Mar. 13
PARKER, PETER	2 Lafayette Square	1871, Mar. 13
Parry, Charles Christopher (a)	Burlington, Iowa	1871, May 13
Patterson, Carlile Pollock (d)		1871, Nov. 17
Paul, Henry Martyn	Naval Observatory. 917 R St. N. W	1877, May 19
Peale, Albert Charles	Geological Survey. 1210 Mass. Ave. N. W.	1874, Apr. 11
PEALE, TITIAN RAMSAY (a)	Philadelphia, Penn	1871, Mar. 13
PEIRCE, BENJAMIN (d)		1871, Mar. 13
Peirce, Charles Sanders (a)	Coast and Geodetic Survey Office. Baltimore, Md.	1873, Mar. 1
Pilling, James Constantine	Geological Survey. 918 M St. N. W	1881, Feb. 19
Poe, Orlando Metcalfe	34 Congress St. West, Detroit, Mich	1873, Oct. 4
Pope, Benjamin Franklin	Surgeon General's Office, U. S. A. 2029 P St. N. W.	1882, Dec. 16
Porter, David Dixon (r)		1874, Apr. 11
Powell, John Wesley	Geological Survey. 910 M St. N. W	1874, Jan. 17
Prentiss, Daniel Webster	1224 9th St. N. W	1880, Jan. 3
Pritchett, Henry Smith (a)	Washington University, St. Louis, Mo.	1879, Mar. 29
Rathbone, Henry Reed (a)		1874, Jan. 17
Rathbun, Richard	Smithsonian Institution. 1622 Mass. Ave. N. W.	1882, Oct. 7
Renshawe, John Henry	Geological Survey. 1221 O St. N. W	1883, Feb. 24
Richey, Stephen Olin	1426 N. Y. Ave. N. W	1882, Oct. 7
Ridgway, Robert (a)	Smithsonian Inst. 1214 Va. Av. N.W.	1874, Jan. 31
Riley, Charles Valentine	Agricultural Dept. 1700 13th St. N.W.	1878, Nov. 9
Riley, John Campbell (d)		1877, May 19
Ritter, William Francis McKnight	Nautical Almanac Office. 16 Grant Place.	1879, Oct. 21
Rodgers, Christopher Raymond Perry (a)	1723 I St. N. W	1872, Mar. 9
Rodgers, John (d)		1872, Nov. 16
Rogers, Joseph Addison (a)	Naval Observatory	1872, Mar. 9
Russell, Israel Cook	Geological Survey. 1424 Corcoran St.	1882, Mar. 25

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Russell, Thomas	Army Signal Office. 904 M St. N. W	1883, Feb. 10
Salmon, Daniel Elmer	Agricultural Dept. 1121 I St. N. W	1883, Nov. 24
Sampson, William Thomas	Naval Observatory	1883, Mar. 24
SANDS, BENJAMIN FRANKLIN (d)		1871, Mar. 13
Saville, James Hamilton	342 D St. (La. Ave.) N. W. 1315 M St. N. W.	1871, Apr. 29
SCHAEFFER, GEORGE CHRISTIAN (d)		1871, Mar. 13
SCHOTT, CHARLES ANTHONY	Coast and Geodetic Survey Office. 212 1st St. S. E.	1871, Mar. 13
Searle, Henry Robinson (d)		1877, Dec. 21
Seymour, George Dudley (r)		1881, Dec. 3
Shellabarger, Samuel	Room 23 Corcoran Building. 812 17th St. N. W.	1875, Apr. 10
Sherman, John	1319 K St. N. W	1874, Jan. 17
SHERMAN, WILLIAM TECUMSEH (r)		1871, Mar. 13
Shufeldt, Robert Wilson	Surgeon Genl's Office, U. S. A. 1619 K St. N. W.	1881, Nov. 5
Sicard, Montgomery (a)	Ordnance Bureau, Navy Department.	1877, Feb. 24
Sigsbee, Charles Dwight	Hydrographic Office, Navy Department. 3319 U St. N. W.	1879, Mar. 1
Skinner, John Oscar	1739 F St. N. W	1883, Mar. 24
Smiley, Charles Wesley	U. S. Fish Commission, 1443 Mass. Ave. 1207 11th St. N. W.	1882, Oct. 7
Smith, David (a)	Navy Department	1876, Dec. 2
Smith, Edwin	Coast and Geodetic Survey Office	1880, Oct. 23
Spofford, Ainsworth Rand	Library of Congress. 1621 Mass. Ave. N. W.	1872, Jan. 27
Stearns, John (a)		1874, Mar. 28
Stone, Ormond (a)	Leander McCormick Observatory, University of Virginia.	1874, Mar. 28
Taylor, Frederick William	Smithsonian Institution	1881, Feb. 19
TAYLOR, WILLIAM BOWER	Smithsonian Inst. 306 C St. N. W	1871, Mar. 13
Thompson, Almon Harris	Geological Survey	1875, Apr. 10
Tilden, William Calvin (a)	Army Medical Museum	1871, Apr. 29
Todd, David Peck (a)	Amherst, Mass	1878, Nov. 23
Toner, Joseph Meredith	615 Louisiana Ave	1873, June 7
True, Frederick William	National Museum	1882, Oct. 7
Twining, William J. (d)		1878, Nov. 23
Upton, Jacob Kendrick (r)		1878, Feb. 2
Upton, William Wirt	2d Comptroller's Office, Treasury Dept. 810 12th St. N. W.	1882, Mar. 25
Upton, Winslow (a)	Army Signal Office. 1441 Chapin St. N. W.	1880, Dec. 4
Vasey, George (r)		1875, June 5
Walcott, Charles Doolittle	Geological Survey. 1116 N. Y. Ave. N. W.	1883, Oct. 13
Waldo, Frank	Army Signal Office. 1427 Chapin St. N. W.	1881, Dec. 3
Walker, Francis Amasa (a)	Mass. Inst. of Technology, Boston, Mass.	1872, Jan. 27
Walling, Henry Francis	Geological Survey	1883, Feb. 24
Ward, Lester Frank	Geological Survey. 1464 R. I. Ave. N. W.	1876, Nov. 18
Webster, Albert Lowry	Johns Hopkins University, Baltimore, Md.	1882, Mar. 25
Welling, James Clarke	1302 Connecticut Ave	1872, Nov. 16
Wheeler, George M. (a)	Engineer Bureau, War Department	1873, June 7
WHEELER, JUNIUS B. (a)	West Point, New York	1871, Mar. 13
White, Charles Abiathar	Geological Survey. LeDroit Park	1876, Dec. 16
White, Zebulon Lewis (a)	Providence, Rhode Island	1880, June 19
Williams, Albert, Jr	Geological Survey. 23 Lafayette Square.	1883, Feb. 24
Wilson, Allen D. (a)	Newport, R. I.	1874, Apr. 11
Wilson, James Ormond	Franklin School Building. 1439 Mass. Ave. N. W.	1873, Mar. 1
Winlock, William Crawford	Naval Observatory. 733 20th St. N.W	1880, Dec. 4
Wolcott, Christopher Columbus (r)		1875, Feb. 27
Wood, Joseph (a)	Asst. Engineer B. & P. R. R.	1875, Jan. 16
Wood, William Maxwell (a)	Navy Department	1871, Dec. 2

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
WOODWARD, JOSEPH JANVIER	Army Med. Museum. 620 F St. N.W.	1871, Mar. 13
Woodward, Robert Simpson	Naval Observatory. 1125 17th St. N.W.	1883, Nov. 24
Woodworth, John Maynard (*d*)		1874, Jan. 31
Yarnall, Mordecai (*d*)		1871, Apr. 29
Yarrow, Harry Crécy	*814 17th St. N. W	1874, Jan. 31
Zumbrock, Anton	Coast and Geodetic Survey Office. 455 C St. N. W.	1875, Jan. 30

Number of *founders*	44	
" members *deceased*		36
" " *absent*		55
" " *resigned*		16
" " *active*		149
Total number enrolled		256

ANNUAL REPORT OF THE TREASURER.

WASHINGTON CITY, *December* 31, 1883.

To the Philosophical Society of Washington:

I have the honor to present herewith my annual statement as Treasurer for the year ending December 31st, and to express my regret that owing to absence from the city I was not able to present this report at the proper time on the occasion of the recent annual meeting, December 22d.

By the kindness of Messrs. Riggs & Co. the Society has been enabled to invest in another $1,000 United States 4 per cent. bond, but in this case a few weeks in advance of the regular winter accumulation of its revenue. The balance shown by Riggs' books against the Society is, therefore, with their assent, and in fact at their suggestion, and will probably be met during January.

The total invested fund of the Society is, therefore, $2,500, of which $1,000 is at 4½ per cent. and $1,500 at 4 per cent.

The further assets of the Society consist of unpaid annual dues to the amount of $185 for 1883 and $90 for 1882. The total active membership remains at about one hundred and fifty, and the probable income for the next year may be estimated at $900, nearly all of which will be needed to pay current expenses and the bills for printing Volume VI.

Early in the year one hundred and fifty-five copies of Volume IV and two hundred and eighty-five of Volume V were distributed to the active members and to about fifty domestic and eighty-five foreign recipients.

The list of recipients is a slightly amended copy of that printed in Volume IV of the Bulletin of the Society. Occasional copies of the earlier volumes have been distributed to those whose sets had accidentally become imperfect, or were otherwise entitled to them.

As custodian of the library and property, I have the honor to report that the Society occasionally receives scientific publications by way of exchange with similar organizations. The accession catalogue of the library now includes a hundred and two numbers or titles, being an increase of thirty-one during the year.

By order of the General Council, the Treasurer was in 1881 instructed to initiate the keeping of a record book containing a sketch of the life and services of the individual members of the Society; this record volume is now prepared, and a notice will be sent to each member asking for the necessary data.

Very respectfully,

CLEVELAND ABBE,
Treasurer.

DR. *The Philosophical Society of Washington in account with Cleveland Abbe, Treasurer, for the year 1883.* CR.

EXPENDITURES.					RECEIPTS.		
Date.	Vou'r.	Check.	To whom paid.	Amount.	From what source.	Amount.	Total.
1883.							
March 1	1	74	Judd & Detweiler, printers	$60 00	Credit by receipts as follows:		
April 21	2	75	Judd & Detweiler, printers	35 50	Balance carried over from Dec. 28, 1882		$521 07
April 21	3	75	Judd & Detweiler, printers	117 08	Annual dues received:		
April 26	4	76	Cleveland Abbe, Treasurer	46 70	and deposited December 23, 1882	$135 00	
June 20	5	77	Judd & Detweiler, printers	11 50	and deposited February 1, 1883	25 00	
Nov. 26	6	78	G. K. Gilbert, Secretary	5 05	and deposited February 6, 1883	125 00	
Dec. 5	7	79	Riggs & Co., $1,000 U. S. 4		and deposited February 14, 1883	50 00	
			per cent.	1,223 75	and deposited March 17, 1883	100 00	
					and deposited May 1, 1883	70 00	
					and deposited November 15, 1883	95 00	
					and deposited November 27, 1883	105 00	
					and deposited December 10, 1883	65 00	
					and deposited December 28, 1883	70 00	
					Interest on invested funds, viz:	$840 00	
					One $1,000 U. S. bond, at 4½ per cent.	45 00	
					One $500 U. S. bond, at 4 per cent.	20 00	
					Total receipts		905 00
					Receipts from all sources		1,426 07
					Over-draft on Riggs & Co.		73 51
			Total	$1,499 58	Total		$1,499 58

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

ANNUAL ADDRESS OF THE PRESIDENT.

ANNUAL ADDRESS OF THE PRESIDENT,

J. W. Powell,

Delivered December 8, 1883.

THE THREE METHODS OF EVOLUTION.

In the early history of research attention was chiefly given to phenomena of co-existence. In late years the phenomena of sequence have received the larger share of attention. The investigation of the phenomena of sequence has led to the invention of a number of hypotheses. In the past history of scientific research three of these have each led to a long series of important discoveries. These are the nebular hypothesis, the atomic hypothesis, and the hypothesis of the development of life. The nebular theory is an hypothesis of astronomic evolution; the atomic theory has gradually assumed the shape of an hypothesis of chemical evolution; and the development theory has been elaborated and re-stated as the hypothesis of biologic evolution. The time has come when in all fruitful research evolution in some form is postulated by each investigator in his own field. Yet many scientific men, though admitting the doctrines of evolution in their own special fields, ofttimes reject them elsewhere; and there is some disagreement even among the greatest thinkers as to the extent to which the hypotheses of evolution can be carried, but all postulate evolution in some form and to some degree.

An attempt will be made in this address to point out what is believed to be the fact—that there are three grand classes of phenomena, constituting three kingdoms of matter and representing three stages of evolution; or, stated in another way, that there has been an evolution of the methods of evolution, so that the methods discovered in the first stage have been superseded by those discovred in the second, and these superseded by the methods of the third stage. It is proposed to indicate and, as clearly as possible within the limits of an address, to define, in terms of matter and motion, the three kingdoms of matter and the three methods of evolution. As precedent to the general statement it will be well, therefore, briefly to consider the kinematic hypothesis.

THE KINEMATIC HYPOTHESIS.

That motion is persistent is the kinematic hypothesis. In the early history of research many modes or varieties of motion were directly observed. To account for these motions they were said to be caused by *forces*, and Force was sometimes defined as that which produces motion. Something, therefore, was conceived to exist—not matter, not motion—an existence that would produce motion. Then arose the question, What is Force—this antecedent of Motion? The researches inaugurated from this standpoint led again and again to the discovery that the antecedent of motion is some other motion, and one after another of the so-called "forces" were thus resolved into motions, until at last only gravity and affinity, and perhaps magnetism, remain as unexplained antecedents of motion. But gravity, affinity, and magnetism are included under one term, "*attraction*," by those who hold that there is yet a *force*—something other than motion which produces motion. Attraction, then, is left. Sometimes these same philosophers speak of "attraction and repulsion." If, then, all forces the actions of which are thoroughly known are resolved into antecedent motions, it is indeed an inductive hypothesis worthy of consideration that the antecedents of the phenomena of attraction and repulsion may also be regarded as modes of motion.

But this hypothesis is reached by another method. It is known that motions may be transmuted from one kind or mode into another. Affinity can be transmuted into motion, and motion into affinity. If we wish to obtain the mode of motion called electricity, we may derive it from mechanical motion through friction, or we may derive it through affinity in the voltaic cell. If we combine a gramme of hydrogen with oxygen, 34,000 units of heat—a mode of motion—are developed. If a gramme of hydrogen be combined with iodine, 3,600 units of heat—a mode of motion—are absorbed. But why introduce single illustrations? A large part of all the powers used by man in the industries of the world are derived from affinity. Affinity, therefore, is the equivalent of motion. By a similar process it is shown that gravity can be transmuted into motion and motion into gravity, and the trasmutation of magnetism into motion and of motion into magnetism is well known.

It is thus seen that while motion may be derived from the so-called forces, gravity, affinity, and magnetism, these so-called force may also be derived from motion. In all other cases where a mode of motion is transmuted, it is but changed into another mode. It is therefore an inductive hypothesis that gravity, affinity, and magnetism are also modes of motion.

This hypothesis is reached by yet another inductive process. There is a vast multiplicity of properties which bodies present to the mind through touch, taste, smell, hearing, and sight—properties at first explained as occult. During the progress of scientific research, one after another of these properties has been resolved into motion, until at last two remain unexplained—rigidity and elasticity. By those who hold with most tenacity to older explanations of such phenomena, these two remaining properties are attributed to attraction and repulsion; but those who have fallen into the current of modern thought believe that they can be explained as the results of the composed motion of the constituent parts of the bodies which exhibit them, together with molecular impact. That some such explanation will eventually be fully established is highly probable as an inductive hypothesis.

When these various methods of induction are combined they lead to an hypothesis of the highest character, and we may reasonably expect that all forces will ultimately be resolved into motions. The term *force* will still be of value in science, to be used in each case as denoting the antecedent motion.

Intimately related to the kinematic hypothesis is the hypothesis of an ether, which has also been reached by a variety of inductive methods, *i. e.* from converging lines of research. In fact, the kinematic hypothesis and the ethereal hypothesis are identical, the first being stated in terms of motion, the second in terms of matter.

Intimately related to the ethereal hypothesis is the nebular hypothesis, also reached through a series of converging lines of induction.

Every fact that lends probability to one lends probability to all. Thus each strengthens the other. It must be understood that however probable they may be, they are yet hypotheses, and for their complete demonstration the mode of action must be specifically pointed out in each case.

The ethereal hypothesis furnishes the original homogeneous matter in motion from which the various aggregates have been segregated. The nebular hypothesis takes up this matter while it is yet in a molecular condition and derives from it the more compounded aggregates and their motions, in obedience to the law of the persistence of motion, which is the kinematic hypothesis. Thus there are bodies of men engaged in researches relating to molecular physics, other bodies of men in researches relating to molecular physics and astronomy, and others in molecular physics and chemistry, all of whose researches converge in the kinematic hypothesis. It is therefore reached by a consilience of many inductive methods.

In the statement thus made concerning the kinematic theory there is no attempt to assemble the data on which it rests. Such task could not be performed in an address, as volumes would be needed for their presentation. An attempt has been made simply to characterize the processes of inductive reasoning by which the hypothesis is reached.

If the kinematic hypothesis should be demonstrated, it would be a veritable explanation. The dynamic hypothesis is no explanation. To exhibit this fact it must be briefly analyzed.

Philosophy is the science of opinion, and the philosopher has for the subject-matter of his science the origin and nature of opinions, and he discovers that they may be broadly grouped in three classes—mythic, metaphysic, and scientific. Mythic opinion arises from the attempt to explain the simple in terms of the compound—that is, to explain biotic and physical phenomena by their crude analogies to human activities. Early man, discovering that his own activities arose from design and will, supposed that there was design and will in all function and motion. Through this method of explanation have arisen the mythologies of the world.

But in the early civilization of the Aryan race a multitude of mythic systems were thrown together and studied by the same body of men, originally for the purpose of deriving therefrom the common truth. The resulting comparison and investigation led to the conclusion that they were all false, and in lieu thereof a new system of explanation was invented. These earlier philosophers of the cities of the Mediterranean, while engaged in the comparison of mythologies, were also engaged in the comparison of languages, and they discovered many profoundly interesting facts of linguistic structure, and the intimate relations between language and thought by which the form of thought itself is moulded. These great facts appearing at the same time that mythic philosophy was dissolving into idle tales, led to the origin of a new philosophic method. The men of that day supposed that the truth is in the word, and that a verbal explanation could be constructed; that the philosophy of the universe could be based on language; and to them verbal statement was explanation, final and absolute, and being was but ideal.

But metaphysic philosophy was displaced by the increase of knowledge—the development of scientific philosophy. In this system the phenomena of co-existence and sequence are objectively discerned and classified.

This bare statement of the three methods can be made more lucid by an illustration. Unsupported bodies above the earth fall, and such phenomena are seen so often as to challenge every man's attention. Early man, whose mind was controlled by mythic opinions, subjectively knew that if he wished to move a body he must push or pull it, and to him there was no other method of originating motion.

Some years ago I was with a small body of Wintun Indians on Pitt River, the chief tributary of the Sacramento, engaged in the study of mythology. I had gone among the rocks for the purpose of awakening echoes, that I might elicit from my dusky philosophers an explanation thereof. Unexpectedly I fell upon an explanation of gravity. We had climbed a high crag, and I sat at the summit of the cliff with my feet overhanging the brink. An Indian near me, who could speak but imperfect English, seemed solicitous for my safety, and said: "You better get out; hollow pull you down." I had previously been intent on watching the operations of his mind for the purpose above mentioned, and this expression seemed to me strange; and it started a line of investigation which I eagerly pursued. I soon discovered that he interpreted the fall of bodies by purely subjective analogies. He who stands on a rock but slightly elevated above the earth feels no fear, but if standing a thousand feet above the base of the cliff, he attempts to look over, fear curdles his blood, and he seems to be *pulled over*. As he climbs a lofty pine, at every increase of altitude there is an increase of fear, and he seems to be pulled down by a stronger force. When he rests upon the solid earth he feels no "pull," but when elevated above it he interprets his subjective feelings as an objective pull. Vacuity is personified and believed to be an actor.

In the early winter of 1882 I was with a party of Indians in the

Grand Cañon of the Colorado. Some of the young men were amusing themselves by trying to throw stones across a lateral gorge. No one could accomplish the feat, though they could throw stones even farther, as they believed, along the level land. Chuar, the chief, explained this to me by informing me that the cañon *pulled the stones down.* The apparent proximity of the opposite wall was believed to be actual, and vacuity was personified and believed to exert a force.

Metaphysic explanations of gravity are found. By that method an absolute up and down is predicated, and bodies have a tendency to fall down. This is an explanation in words, the words expressing no meaning but believed to be themselves thoughts. It is perhaps the earliest form of the metaphysic explanation of gravity. But with the progress of knowledge the absolute disappears, and positions are found to be but relative; there is no absolute up and down; and other facts with regard to gravity are discovered. And finally the metaphysician says bodies *attract.* Now the term *fall,* as used by the early metaphysicians, was the name of a motion observed, and it was held to be a complete explanation as long as up and down was supposed to be absolute, not relative; and the explanation was abandoned as insufficient when the ideas of absolute up and down were abandoned. But the word *attraction* does not involve this error. It is simply a name for the phenomenon, without the manifestly fallacious implication of "up and down." And it is a good name for the specific phenomenon to which it is applied. But it must not connotate any other idea; in so far as it does, it is vitiated. Yet the metaphysician will suppose that by using the term "attraction" he explains gravity. The scientific philosopher uses the term purely as the name of the phenomenon, and does not suppose that thereby the phenomenon is explained; and having named it, he still seeks for its explanation—that is, he still seeks to resolve that which is manifestly a complex phenomenon, exhibited in the relations of positions of bodies, into its most simple elements. Whenever this is done he will say that attraction, or gravity—they being synonyms for the same phenomenon—is explained.

The kinematist uses "attraction" as a synonym for "gravitation." The dynamist uses "attraction" as a verbal explanation of Gravitation. The mythic philosopher uses the term to connotate the still further idea that bodies exert a "pull" on one another; and this latter concept is no less mythic than that of the Indian who believes that the vacuity between them exerts the pull.

It is fortunate for science that every discovery and every inductive hypothesis is rigidly criticised, as this leads to the careful examination of the verity of facts discerned and of the legitimacy of hypotheses derived therefrom. Against the kinematic theory of force much good rhetoric has been hurled, which may be somewhat imitated in the following manner:

Here is a quotation from Bagehot, with an interpolation of my own: "This easy hypothesis of special creation [occult force] has been tried so often, and has broken down so very often, that in no case probably do any great number of careful inquirers very firmly believe it. They may accept it provisionally, as the best hypothesis at present, but they feel about it as they cannot help feeling as to an army which has always been beaten; however strong it seems they think it will be beaten again."

The venerable gentlemen who constitute the elder school tell us that motion is not persistent; that *energy* constitutes a class of things including two groups, the forces on the one hand and the motions on the other; that the total amount of energy is persistent, but that the total amount of motion is changeable. And by their definition force is that which produces motion, *i. e.* force can create or destroy motion. But manifestly where there is more motion there must be less force, therefore force can destroy itself; and when there is more force and less motion, force can create itself.

The moon that passes through the sky of the gentlemen of the old school is moon from the eastern to the western horizon. Then the dragon, which exists not, destroys the moon and thus creates itself, and passing through the cave from west to east it mounts to their horizon, and in the twinkling of an eye commits suicide by creating a moon. It is not strange that the thaumaturgics of such philosophy should lend signal aid to its rhetoric.

The use of hypothesis in science is not only legitimate but an absolute necessity. The science of psychology, as distinguished from metaphysic speculation, points out this fact: that all increase of knowledge is dependent upon hypothesis. Objective impressions made by the phenomena of the universe upon the organ of the mind are discerned only by the aid of comparison, and are added to knowledge only by being combined with previously discerned phenomena. Phenomena imperfectly discerned are such as are combined by superficial analogies; phenomena clearly discerned are such as are combined by essential homologies. With all discernment, therefore, there is comparison, and comparison is reflection and reflection is reason. Now, scientific research is not random observation and comparison, but designed discernment and classifition; it is research for a purpose, and the purpose is the explanation of imperfectly discerned phenomena. Phenomena not understood, because imperfectly discerned and classified, are made the subject of examination by first inventing a hypothetic explanation of the same. With this, the investigator proceeds to more careful observation and comparison, devising new methods of discrimination and of testing conclusions. Under the impetus of this hypothetic explanation, discernment and comparison proceed, and additions to knowledge are made thereby, and it matters not whether the hypothesis be confirmed or overthrown.

On this rock much research is wrecked. When an hypothesis gains such control over the mind that phenomena are subjectively discerned, that they are seen only in the light of the preconceived idea, then research but adds to vain speculation. A mind controlled by an hypothesis is to that extent insane; the rational mind is controlled only by the facts, and contradicted hypotheses vanish in their light.

There is another rock on which research is wrecked—the belief which ofttimes takes possession of the mind that the unknown is unknowable, that human research can penetrate into the secrets of the universe no farther. It is the despondency of unrewarded mental toil.

Yet another rock on which research is wrecked is the definition of the unknown. Phenomena appear, but whence is not discovered, and resort is had to verbal statement, and the verbal statement oft repeated comes to be held as a fact itself. This is the vice of all metaphysics, by which *words* are held to be *things*—spectral imaginings that haunt the minds of introverted thinkers as devils possess the imaginations of the depraved.

In the midst of the sea of the unknown stand the three rocks: the controlling hypothesis, the unknowable unknown, and the verbal definition, and in the waters about them are buried many wrecks.

COMBINATION OF MATTER.

When the various bodies known to mind are resolved into their constituent parts to the utmost of art and knowledge, such parts are found to be so minute as almost to disappear in the perspective toward the infinitesimal. The molecular bodies thus dimly discerned are combined and re-combined, until substances are produced that come distinctly within the cognizance of our senses, so that we are able to observe their forms and motions. These molar bodies are again combined, until at last bodies of such magnitude are produced that they are but dimly discerned in the perspective toward the infinite—stellar systems that appear not to the eye, but only to the mind's eye.

INORGANIC COMBINATION.

Matter is primarily combined by chemical affinity. The sub stances thus produced appear in three states: gaseous, fluid, and solid, but are not clearly demarcated. That chemically combined matter which is found in the solid state is further combined by crystallization and lithifaction. It may be that these methods are parts of the same process, and further, that they are one with chemical affinity; at any rate it is impossible clearly to demarcate them. They are also influenced by gravity, and to a large extent act under its control. Thus it is that gravity, and affinity with its concomitants, unite in molecularly combining matter into inorganic substances. Again, these bodies are mechanically combined into geologic formations, bodies of water, and bodies of air, and such combinations result from gravity. Finally they are all combined into an aggregate, the earth itself, solid, fluid, and gaseous. This also results from gravity.

In the succession of combinations thus briefly reviewed, the first natural aggregate reached is the earth. Below that we have chemical and mechanical substances, which do not constitute integers, but only integral parts. The earth itself is a whole—an aggregation, as the term is here used.

Again, the earth is one of the bodies of the solar system, which is a combination of worlds. This aggregation, also, is controlled by gravity. Other higher astronomic aggregates may exist.

ORGANIC COMBINATION.

Portions of the matter combined by affinity and gravity are segregated to be combined by vitality, giving organic bodies or aggregates, as plants and animals. These bodies do not permanently remain such, as the matter of which they are composed sooner or later returns to the condition of combination due solely to affinity and gravity. They live and die.

SUPERORGANIC COMBINATION.

There are certain biotic bodies whose activities are combined. The first step in combination is the biologic differentiation of the sexes, giving a group of co-operative individuals for the activities of reproduction—male and female, parent and child. This initial combination is crudely developed into still larger combinations of co-operative individuals among the lower animals. With mankind it is developed to a much higher degree, resulting in a great variety of co-operative activities.

There is found, then, a variety of methods of combination, included under three classes: physical, due to affinity and gravity; biotic, due to vital organization; and anthropic, due to related activities. Physical combinations result in the production of *substances* and *aggregates*, and the existence of a physical body is preserved by preserving identity of form and identity of constituent matter. Biotic combination also produces substances and aggregates, and the existence of a biotic body is continued by the preservation of identity of form, but not of identity of constituent matter. In anthropic combination, substances and aggregates, as the terms are here used, are not produced, but biotic aggregates are interrelated in their activities through the agency of mind.

In physical aggregates the relation of parts is that of interdependence, so that the constitution and form of each part are dependent on the constitution and form of every other part. This interdependence may be better comprehended by means of an illustration. In the aggregate the earth, the interdependence is exhibited in the relations existing between the incompletely aggregated bodies of minerals, known as geologic formations; the incompletely aggregated bodies of water, known as seas, lakes, streams, and clouds; and the incompletely aggregated bodies of air, known as winds. Air-currents gather the waters from the seas and pour them upon the lands. Rains and rivers disintegrate the rocks and carry them to the sea. Currents in the sea distribute the detritus over the bottom. By the loading of areas of sea-bottom they are depressed, and by the degradation of land-areas they are unloaded and rise. Change in the geography of the land effects a change in wind-currents and in bodies of water, and a change in the latter effects a change in sedimentation. In like manner, throughout all physical nature, an interdependence of parts is exhibited. Part acts *on* part.

In biotic aggregates the same interdependence of parts is shown. Any change affecting the digestive apparatus affects the circulatory apparatus, and these again are influenced by the respiratory apparatus. But in addition to this interdependence of parts, there is also an organization of parts—that is, special functions are performed by the several parts, and each is the organ of its function. And this organization is of such a nature that each works for the others. The digestive apparatus digests for itself and all the organs, the heart propels for all the body, the eye sees for all the body, the ear hears for all the body, the hand touches for all the body. Thus the organic parts act *on* and *for* one another.

In activital combination, aggregates, as the term is here used, do not appear, but the same interdependence is observed. By association the sanitary state of the husband affects that of the wife, and the condition of the mother affects the child; and on through the different combinations of animals and men this interdependence is observed. The relation of organization also exists by the differentiation of industries. The husband brings food to the wife and children, and the wife prepares the food. And this differentiation of industries, or "division of labor" as it is termed in political science, is carried on to an elaborate condition in civilized life. Then men are related to one another as constituent members of society; one commands and another obeys. Then men are related to one another through language; one speaks, another hears; one writes, another reads. Then men are related to one another through opinions; having common opinions, they form common designs and act for common purposes. It will thus be seen that superorganic or anthropic combination arises from the establishment of four classes of relations, corresponding to the four classes of activities represented by arts, institutions, languages, and opinions. The arts are human activities directed to the utilization of the materials of nature and the control of its powers, for the purpose of securing happiness. Institutions are human activities arranged for the purpose of securing peace and establishing justice, and thereby increasing happiness Languages are activities devised for the purpose of communicating thought, and thereby securing happiness. Opinions arise from psychic activities, the purpose of which is to learn the truth, that happiness may ensue.

In physical, biotic, and anthropic combinations the parts control one another. It will therefore be convenient to speak of three kingdoms of matter: the mineral or physical kingdom, the organic or biotic kingdom, and the anthropic or activital kingdom.

MODES OF MOTION.

All bodies, however combined, are discovered to be in motion.

Among the bodies of the mineral kingdom, a variety of modes of molecular motion are exhibited, having various distinguishing characteristics. These are heat and light, electricity and magnetism, then sound and that motion in gases by which through impact they retain their rarefied state. Again, a variety of molar motions are observed in gases, liquids, and solids; and finally stellar motions are observed in astronomic systems.

In the biotic kingdom plants and animals exhibit many varieties of organic motions, called *functions*. These are superadded to the physical motions, which appear alike in the physical and biotic kingdoms. Physical bodies exhibit motions; biotic bodies exhibit motions and functions, the latter being highly organized motions.

In the anthropic kingdom there is a complexity of motions arising from biotic functions, which are arranged and combined so as to produce *activities*. These activities are represented by arts, institutions, languages, and opinions.

Thus there are three great classes of motions corresponding to the three great classes of combinations, namely, physical motions; biotic motions, or functions; and anthropic motions, or activities.

THE RELATION OF MOTION TO COMBINATION.

It will at once be seen that anthropic combination is such by virtue of human activities. Activital combination is manifestly composed motion.

Again, biotic aggregates are such by virtue of continuous combination and dissolution. Within proper limits a biotic body may be compared to a river; it is a form through which matter passes. In plants some of this passing matter becomes fixed for a time, but eventually returns from the biotic to the mineral kingdom. Among animals this passage of physical matter through the biotic form is more rapid. The organic functions, also, of these bodies are but arranged or organized motions. Life is motion—the specific motion called *function.*

Again, among the aggregations of the physical kingdom, stellar systems are aggregates by virtue of motion. The combination observed is due to composed motion. Of the mechanical combinations, that exhibited in the atmosphere is such by virtue of motion—that is, the gaseous state is preserved by the interference of molecular motions, and the bodies into which it is imperfectly differentiated, *i. e.*, currents of air, are such by virtue of motion. Again, the imperfectly aggregated bodies of water are such by virtue of motion. This is seen to be true of the clouds floating in the air, and of rivers rolling to the seas. Lakes with outlets are bodies of water in motion, forever fed from the clouds, forever discharging into the sea; and mediterranean seas without outlet are perpetually receiving and discharging their waters; and so far as the sea is differentiated into currents, these are bodies imperfectly aggregated by motion.

There yet remain certain molecular combinations of inorganic substances, due to affinity and gravity, the nature of which is not so immediately perceived. Now, as all societies and other anthropic combinations are such by virtue of their motions, known as activities, and as all biotic bodies are such by virtue of their functions, and as all stellar combinations are such by virtue of stellar motion, and as finally all mechanical combinations are such by virtue of motion, it is at once suggested as an inductive hypothesis that those combinations the nature of which is yet unknown are also such by virtue of motion. It is an hypothesis worthy of consideration, that affinity and gravity are also due to motion. It has even been supposed by some that chemical and barologic methods of combination are but diverse modes of the same process; that affinity and gravity constitute but one method of combination, and that we call it *affinity* when the combination involves minute bodies, below our sense perceptions, and *gravity* when larger bodies are involved.

An attempt has thus been made to define the three kingdoms of matter in terms of matter and motion, showing that there are three methods of combination, and that the parts combined are related by three corresponding methods, and that in each kingdom motions of a distinctive class are discovered. The constitution of physical bodies is due to composed motion; the constitution of biotic bodies is due to composed transmutations of motion; anthropic combinations are due to related activities.

In order that there be evolution, there must be change in combination of matter and in mode of motion. The sole property of matter is motion, and motion itself is change of position. But this change of position results in change of combination, and change of combination results in change of mode of motion. These changes must now be set forth.

CHANGE OF COMBINATION.

If the mind could discern and classify all the bodies of the universe at any one moment, only space conditions would enter therein; but bodies change from time to time, so that there are sequences of combination. Substances and aggregates of matter are such by reason of an arrangement in position of their constituent parts. Substances and bodies change in external relations and in internal relations. Change in external relations is change of position in relation to external things. Change in internal relations is the change in relative arrangement of constituent parts. And this change of position is always motion, the first and only property of matter.

Chemical, crystalline, and lithical combinations are decomposed and otherwise re-composed, mechanical combinations are broken up and otherwise re-arranged, and stellar aggregates are believed to have been gradually formed. With physical bodies internal change is the direct result of external change. This is their distinctive characteristic, that all their changes of constitution result directly from agencies without themselves.

Biotic bodies exhibit the same changes as mineral bodies, and also a series peculiar to themselves. First, biotic substances are segregated from the mineral kingdom—*i. e.*, mineral substances are changed into biotic substances. Second, biotic bodies begin, grow, decline, and die. This is a progressive change of structure. Third, the structure of biotic bodies is preserved by continuous change in their constituent matter. Form and structure are preserved while the matter is forever changing. Life is a determined, systematic sequence of transmutations of motion, transformations of matter, and transfigurations of body. Life is change. Fourth, as the individuals are not persistent, the method of aggregation continues by the processes of reproduction of like forms. But these like forms are made unlike—*i. e.*, changed—by two processes. In the biotic reproduction of the higher forms the bisexual method prevails, so that each individual is the offspring of two parents, like both so far as they are alike, but differing from the one or the other so far as they are unlike. Fifth, the individual has its constitution determined by its parents, but subject to changes which may be brought about by external relations differing from those to which the parents were subjected; and within limits these are transmitted to offspring. Thus it is seen that biotic changes are caused by external and internal agencies.

This may be put in another form. In mineral bodies the same matter is changed in structure. In biotic bodies the same or nearly the same structure remains and the constituent matter changes; yet there is a slow change in structure from birth to death, and a still further change in structure from generation to generation; but there is more rapid change of constituent matter. Anthropic aggregates arise, not by a combination of matter, but by a combination of the activities of biotic bodies. These biotic bodies themselves change, as individuals disappear and new ones take their places. Thus family group succeeds family group, and generations of people succeed generations of people. In the same manner arts change. Old arts are abandoned and new arts appear. Various societies cease to exist and new societies are organized. The organization due to the differentiation of operations steadily increases by the division of labor; and the grouping of bodies of men into states, *i. e.*, tribes and nations, is in constant flux. So, languages change—they grow and die. And opinions change with each individual and from generation to generation. All these changes are determined by the will of the individual units who are actors—that is, activities change because the actors so desire. Anthropic change is due to psychic agencies.

CHANGE OF MOTION.

That motion is persistent is a fundamental axiom. But while it does not change in quantity it changes in quality in diverse ways. First, motion may be changed in direction. Simple motion is the motion of a body in a straight line, and change of such motion of the lowest order is change in direction, and this is accomplished by the combination of two or more motions having different directions. Then motion may be transmitted from one body to another. The molecular motions—heat, light, electricity, sound, etc.—are motions propagated by transmission from molecule to molecule. In the kinematic hypothesis of gravity it is held that atomic motion is transmitted from atoms to combined and aggregated bodies by impact; and here we reach another method of change—that by transmutation. One mode of motion may be transmuted into another, as molar motion into heat, and heat into electricity.

By the combination of matter motion is composed. Mineral substances and aggregates exhibit this composition of diverse modes of motion. Biotic bodies exhibit composition of modes of motion, and also composition of transmutations of motion, and it is this latter characteristic which distinguishes biotic from physical motion. Activital combinations exhibit a composition of modes of motion, and a composition of the transmutations of motion, and a composition by co-operative action. It is the last characteristic which distinguishes activital motion from biotic.

The changes of motion exhibited in the mineral kingdom are changes in direction by combination, changes in relative quantity by transmission, changes in mode of motion through transmutation, and changes in the combination of modes of motion.

In the biotic kingdom the same changes are found as in the mineral kingdom, but to them are added changes in the composition of transmutations of motion.

In the anthropic kingdom all the changes in the other kingdoms appear, together with changes in the composition of activities.

EVOLUTION DEFINED.

As matter is indestructible, when one combination or aggregation is dissolved some other must appear, and *vice versa*. Existing bodies must have antecedents. In tracing backward the history of bodies, lines of sequences are followed. Many such are known, and the first important characteristic to be noted of them is *they are orderly*. Like bodies have like antecedents. From this results one of the highest inductions of science, namely, that from consequents antecedents can be restored, and from antecedents consequents can be predicted. The second important characteristic of these sequences of change is that *many are in a definite direction*, which is gradually becoming known. This general course of change is denominated Evolution, and the term must be defined.

Evolution is progress in systemization. It must be noted that not all changes are progressive; some are retrogressive. It is only progressive change that is here called evolution; retrogressive change is dissolution. As the term is here used, a System is an assemblage of interdependent parts, each arranged in subordination to the whole so as to constitute an integer. Evolution may therefore be defined in another way. It is progress in differentiation by the establishment of unlike parts, and in the integration of these parts by the establishment of interdependence. Dissolution is retrogression by the lapsing of integration through the destruction of interdependence, and the lapsing of differentiation through the loss of heterogeneity in parts.

EVOLUTION IN THE PHYSICAL KINGDOM.

Under the kinematic hypothesis, which embraces the ethereal and nebular hypotheses, portions of discrete matter have been segregated to be combined and aggregated. The process precedent to evolution, then, is combination and aggregation, by which substances and integers are produced.

Whatever may be the fate of the explanation of the origin of substances and aggregates through the kinematic and concomitant hypotheses, the fact remains that such bodies exist, and the evolution of matter, as it is hereafter dealt with, starts from this point. Given substances and aggregates as they are known to exist in nature, and given changes which they are known to undergo, it is proposed to point out by what methods evolution is attained.

The terms *substance* and *aggregate* have been used as distinguishing two orders of combination. It should be noted that they cannot be clearly demarcated. Substances are composed of homogeneous, non-interdependent parts, but this homogeneity is never absolute, and some slight degree of interdependence may always be discovered. Aggregates, on the other hand, are composed of heterogeneous, interdependent parts, but degrees of heterogeneity and interdependence appear. Combination is the bringing together of dissociated matter; and it is in the combinations, separations, and re-combinations of matter that evolution appears.

In mineral bodies combinations proceed by molecular, molar, and stellar methods. It has been shown that the changes in these bodies are due to external conditions or forces. If a given body be in harmony with external conditions no change occurs in its constitution, but if it be out of harmony the impinging agencies effect such modifications as will produce harmony. This may be done by a change in the body as a substance or aggregate, or by its separation and re-combination in some more harmonious form. The evolution of mineral bodies is thus accomplished by direct adaptation to external conditions.

If it is permitted hypothetically to conceive of a universe of ethereal matter—*i. e.*, matter composed of discrete atoms in motion, such atoms would remain in an attenuated condition by atomic impact. In matter thus constituted, motion could be transmitted from atom to atom, but no new mode of motion would result therefrom. The mass of matter thus constituted would be absolutely homogeneous. But if by some method several such atoms should be combined, so as to move together as a common body, and so that their interspaces could not be penetrated by other atoms, the motion of an impinging atom would not only be transmitted to the larger body, but it would also be transmuted into another mode or kind of motion. If other such molecules were formed by the segregation of atoms from the homogeneous mass, the new kind of motion would be set up in all the matter thus segregated, and the motions of these bodies would react one upon another. If, again, some of these molecules were segregated, to be combined in larger bodies, with or without such a diminution of interspaces as to prevent the interpenetration of atoms, a third mode of motion would be established; and if diverse methods of aggregation should occur, diverse modes of motion would be established thereby; and in all combining and re-combining, aggregating and re-aggregating, new modes and complexities would arise.

It is a well-known law that a moving body passes in the direction of the least resistance. Diverse modes of motion may exist in a body, due to the complexities of its organization. In the transmission of motion to such a body from another by impact, the motion transmitted is transmuted into that mode which gives it the least resistance. This is illustrated on every hand. When a smaller body impinges against a larger, the inequality between the two may be so great that molar motion is not set up in the larger body, but the whole of the imparted motion is transmuted into heat or some other molecular motion.

This law, that motion passes in the direction of least resistance, is the equivalent of the law of adaptation in the evolution of matter. When evolution is considered from the standpoint of matter, it is convenient to use the term *Adaptation;* when considered from the standpoint of motion, it is more convenient to use the term *Least Resistance.*

EVOLUTION IN THE BIOTIC KINGDOM.

In biotic bodies it has been seen that change is the result of internal as well as external conditions. As external conditions, or the environment, are changing, these bodies change to a limited extent, in the same manner as do mineral bodies; but there is also a change brought about indirectly by the environment, through certain internal changes in the constitution of biotic bodies. Through this internal constitution individuals are changed in time—one generation dies and another succeeds.

There is yet another method of change in biotic bodies, which steadily increases from the lowest to the highest—that is, the change in their constituent matter. While structure changes slowly from birth through growth and decadence to death, the constituent matter changes with much greater rapidity. In this change the minute elements of structure change much more rapidly than the larger into which they are compounded; so that every part of the organ must be supplied with new material to replace that which is steadily becoming effete and passing away. Now the rate of this change in any integral part of an organism is dependent upon the activity of the organ. Exercise increases the rate of change in the constituent matter of a biotic organ, and thus the slow change in its structure, which proceeds from life to death, is accelerated. This accelerated change results in increased differentiation of the organ, and it thereby becomes more and more efficient in the performance of its function. This change, therefore, results from exercise. Organs that are exercised increase in efficiency, by non-exercise they decrease in efficiency. This change in the organization of any one individual is but slight, but as the slight changes pass from one generation to another, continuous exercise of one set of organs greatly modifies them; continuous neglect of exercise in another set modifies them also, until at last they are atrophied. Thus by exercise and non-exercise important structural changes are produced when conjoined with the changes due to heredity.

All these changes result in progress, from the fact that those individuals whose change is in a direction out of harmony with the environment ultimately perish, while those whose change is in a direction in harmony with the environment survive. This method of adaptation or evolution in biology is called "the survival of the fittest."

The rate of evolution by survival is greatly accelerated by another condition. Each pair of biotic bodies reproduce a large number of new bodies, so that reproduction from generation to generation is in a high geometric ratio. The earth having become occupied with all the biotic beings that can derive sustentation therefrom, but a small fraction of the beings produced in a generation can live. Few survive, many succumb. Survival by adaptation is therefore made more efficient by competition.

There are other changes in the biotic kingdom brought about by adaptation. The multiplicity of biotic beings, causing over-population, has crowded them into every conceivable habitat—in the air, on the land, and in the water; and living beings have become adapted thereto by the development of wings, legs, fins, and correlative organs. Thus by exercise organs have been developed, and by non-exercise other organs have been atrophied, until living beings have become specialized for a vast diversity of habitats—for life on the mountain and in the valley, in the light and in the dark, in the cold and in the heat, in humid regions and in arid regions. Living beings have also been adapted to various kinds of food and to various methods of acquisition—in fine, to a great variety of conditions.

This specialization by development, through exercise and non-exercise, must be clearly distinguished from the processes of evolution. The heterogeneous living beings thus produced are but multiplied and diverse forms, animals and plants alike being as often degraded as evolved in the processes of specialization. Degradation is especially to be noticed in parasitic animals and others adapted to extremely abnormal habitats; but it should be understood that a form thus produced may, in the process of its production and subsequent existence, make progressive change in the system of its structure by the methods of evolution already characterized.

Specialization is greatly accelerated by a peculiar method. As all the higher animals are physically discrete, psychic relations must be established, in order that they may meet for the act of reproduction. These psychic relations gradually develop into choice, or sexual selection, and by methods which have been clearly pointed out by biologists the minute increments of change that result therefrom eventually accumulate into strong variations, always adapted to the conditions of the environment. Thus the survival of the fittest is accelerated by sexual selection.

EVOLUTION IN THE ANTHROPIC KINGDOM.

If attention is directed exclusively to animal life, we notice that evolution has proceeded *pari passu* with specialization. Of the forms that have been specialized from time to time some have become extinct, some have been degraded, and some have been evolved in varying degree. One form, not the most specialized, made the greatest progress in evolution, until an organism was developed of so high a grade that this species became more independent of environment than any other, and, by reason of its superiority, spread widely throughout the land portion of the globe This superior animal was early man, when he first inhabited all the continents and the great islands. The production of this superior, *i. e.* more highly systematized organism, was the antecedent to the inauguration of new methods of evolution.

It has been shown that the great efficiency of the biotic method of evolution by survival depends upon competition for existence in enormously overcrowded population. Man, having acquired superiority to other animals, passed beyond the stage when he had to compete with them for existence upon the earth and into the stage where he could utilize plants and animals alike for his own purposes. They could no longer crowd him out, and to that extent the law of the survival of the fittest in the struggle for existence was annulled in its application to man. He artificially multiplies such of the lower animals as are most useful to him, and domesticates them, that they may be more thoroughly under his control, and modifies them, that they may be more useful, and uses such as he will for beasts of burden; and the wild beasts he destroys from the face of the earth. In like manner he cultivates useful plants, and destroys such as are worthless to him. He does not compete with other biotic species, but utilizes them for his welfare. Yet the law of the survival of the fittest applies in so far as it is not dependent upon competition, and slow evolution may still result therefrom. But at this stage new methods spring up of such great efficiency that the method by the survival of the fittest may be neglected because of its insignificance.

In anthropic combinations the units are men, and men at this stage are no longer passive objects, but active subjects; and instead of man being passively adapted to the environment, he adapts the environment to himself through his activities. This is the essential characteristic of anthropic evolution. Adaptation becomes active instead of passive. In this change certain parts of the human organism are increasingly exercised from generation to generation. This steadily increasing exercise results in steadily increasing development, and the progress of the unit—man—in this higher organization depends upon development through exercise. But the progress by exercise depends upon the evolution of activities.

Man is an animal, and may be studied as such; and this branch of science belongs to biology. But man is more than an animal. Though an animal in biotic function, he is man in his anthropic activities; for by them men are combined—*i. e.*, interrelated—so that they are not discrete beings, but each acts on, for, and with, his fellow-man in the pursuit of happiness. Human activities, thus combined and organized, transcend the activities of the lower animals to such a degree as to produce a new kingdom of matter. The nature of these activities must here be set forth.

The first grand class is composed of those which affect the external world, and by them men are interrelated through their desires. These activities are the Arts. The arts have been evolved by human invention, and man has been impelled thereto by his endeavor to supply his wants. In the course of the evolution of the arts, man has progressively obtained control over the materials and powers of nature. All the arts of all the human period are the inventions of men. But invention has proceeded by minute increments of growth. A vast multiplicity of arts have been devised, of which comparatively few survive in the highest civilization. As the inventions have been made, the best in the average has been chosen. Man has therefore exercised choice. The evolution of the arts has thus been by the method of invention and choice, in the endeavor to gratify desire, and by them man has adapted the environment to himself.

Second. There is a grand class of activities through which men are interrelated in respect to their conduct. These activities result in Institutions. Through them men are associated for a variety of purposes. Every institution is an organization of a number of individuals, who work together for a common purpose, as, for example, to prosecute some industrial enterprise, to co-operate in the pursuit of pleasure, to promote some system of opinions, or to worship together under the forms of some religion. All such institutions constitute a class denominated Operative Institutions. A second class are the institutions which man has organized for the direct regulation of conduct. These are States and their subordinate units, with their special organs of government, and rules for the regulation of conduct, called Laws.

Institutions have been developed from extreme simplicity to extreme complexity. They are all the inventions of mankind, and their evolution has been by minute increments of growth. Their invention has been wrought out that men might live together in peace and render one another assistance; and gradually, by the consideration of particulars of conduct as they have arisen from time to time, men have sought to establish justice, that they might thereby secure peace. Of the vast multiplicity of institutions—forms of state, forms of government, and provisions of law—which have been invented, but few remain in the highest civilization, and these few have been selected by men. Men have thus exercised choice. Institutions, therefore, have been developed by invention and the choice of the just in the endeavor to secure peace.

Third. There is another fundamental group of activities through which men are interrelated in respect to their thoughts. These are the activities of mental intercommunication, and result in Languages. Languages, also, are inventions by minute increments of growth. Many languages have been invented, and in each language many words and many methods of combining linguistic devices have been invented. In the languages of the most civilized peoples, but few of these survive; and there are spoken by all the peoples of the earth but few languages in comparison to the many that existed in the early history of mankind; and the method of survival, when analyzed, is found also to be choice. Men have chosen the economic in the expression of thought. Languages, therefore, have developed by invention and choice in the struggle for expression.

Fourth. There is a grand class of activities by which men are interrelated in respect to their designs. Men arrive at Opinions, and these have always reacted upon languages, institutions, and arts, and largely led them in their courses of progress. Because of their opinions, men are willing to work together, and thus have common designs. There have been many opinions and many systems of philosophy. Of all that have existed, but few remain in the highest civilization. A careful analysis of the facts relating to the growth of opinions reveals this truth, that opinions also are invented, and that the final survival of the few has been due to the human act of choice in the selection of the truth. Opinions, therefore, have been developed by invention and choice in the struggle to know.

Fifth. Opinions are formed as the direct activities of the Mind. Languages, institutions, and arts have arisen through the action of the mind and the exercise of other corporeal functions. All these activities, therefore, are dependent upon the mind. On the other hand, these objective activities react upon the mind, so that mental operations are controlled thereby. Through the exercise of the mind in the prosecution of activities it is developed. These mental activities are perception and comparison, or reflection, as it is more usually called. The subjective evolution of the mind is therefore the product of the objective evolution of activities.

These five great classes of activities are interdependent in such a manner that one is not possible without the others; they arise together, and their history proceeds by a constant interchange of effects. All the five classes of activities react upon man as an animal in such a manner that his biotic history subsequent to his differentiation from the lower animals is chiefly dependent thereon. The evolution of man as a being superior to the beast is therefore due to the organization of activities.

It has been shown that man does not compete with the lower animals for existence. In like manner, man does not compete with man for existence; for by the development of activities men are interdependent in such a manner that the welfare of one depends upon the welfare of others; and as men discover that welfare must necessarily be mutual, egoism is transmutted into altruism, and moral sentiments are developed which become the guiding principles of mankind. So morality repeals the law of the survival of the fittest in the struggle for existence, and man is thus immeasurably superior to the beast. In animal evolution many are sacrificed for the benefit of the few. Among mankind the welfare of one depends upon the welfare of all, because interdependence has been established.

It has thus been shown that there are three stages in the combination of matter and motion, and that each stage is characterized by a clearly distinct method of evolution. These may be defined as follows:

First, physical evolution is the result of direct adaptation to environment, under the law that motion is in the direction of least resistance.

Second, biotic evolution is the result of indirect adaptation to the environment by the survival of the fittest in the struggle for existence.

Third, anthropic evolution is the result of the exercise of human faculties in activities designed to increase happiness, and through which the environment is adapted to man.

These may be briefly denominated: evolution by adaptation, evolution by survival of the fittest, and evolution by endeavor.

Civilized men have always recognized to some extent the laws of human evolution,—that activities are teleologically developed, and that happiness is increased thereby. In the early history of mankind the nature of teleologic endeavor was so strongly impressed upon the mind that the theory was carried far beyond the truth, so that all biotic function and physical motion were interpreted as teleologic activity. When this error was discovered, and the laws of physical and biotic evolution established, vast realms of phenomena were found to have been entirely misunderstood and falsely explained, and teleologic postulates have finally fallen into disrepute. Men say there is progress in the universe by reason of the very laws of nature, and we must let them alone. Thus, reaction from the ancient false philosophy of teleology has carried men beyond the truth, until they have lost faith in all human endeavor; and they teach the doctrine that man can do nothing for himself, that he owes what he is to physical and biotic agencies, and that his interests are committed to powers over which he has no control.

Such a philosophy is gradually gaining ground among thinkers and writers, and should it prevail to such an extent as to control the actions of mankind, modern civilization would lapse into a condition no whit superior to that of the millions of India, who for many centuries have been buried in the metaphysical speculations of the philosophy of ontology. When man loses faith in himself, and worships nature, and subjects himself to the government of the laws of physical nature, he lapses into stagnation, where mental and moral miasm is bred. All that makes man superior to the beast is the result of his own endeavor to secure happiness.

Man, so far as he is superior to the beast, is the master of his own destiny, and not the creature of the environment. He adapts the natural environment to his wants, and thus creates an environment for himself. Thus it is that we do not discover a biotically aquatic variety of man, yet he dwells upon the sea and derives sustentation from the animals thereof by means of his arts. A biotically arboreal variety of man is not discovered, but the forest are used in his arts and the fruits of the forests for his sustentation. An aërial variety of man is not discovered, but he uses the winds to propel his machinery and to drive his sails; and, indeed, he can ride upon the air with wings of his own invention. A boreal variety of man is not discovered, but he can dwell among the everlasting snows by providing architectural shelter, artificial warmth and bodily protection.

Under the influences of the desert a few plants secure a constitution by which the moisture imbibed during brief and intermittent rains is not evaporated; they become incrusted with a non-porous glaze, or contract themselves into the smallest space and exist without life until the rain comes again. Man lives in the desert by guiding a river thereon and fertilizing the sands with its waters, and the desert is covered with fields and gardens and homes. Everywhere he rises superior to physical nature. The angry sea may not lash him with its waves, for on the billows he builds a palace, and journeys from land to land. When the storm rises it is signaled from afar, and he gathers his loved ones under the shelter of his home, and they listen to the melody of the rain upon the roof. When the winds of winter blow he kindles fossil sunshine on his hearth, and sings the song of the Ingleside. When night covers the earth with darkness he illumines his path with lightning light. For disease he discovers antidote, for pain nepenthe, and he gains health and long life by sanitation; and ever is he utilizing the materials of nature, and ever controlling its powers. By his arts, institutions, languages, and philosophies he has organized a new kingdom of matter, over which he rules. The beasts of the field, the birds of the air, the denizens of the waters, the winds, the waves, the rivers, the seas, the mountains, the valleys, are his subjects; the powers of nature are his servants, and the granite earth his throne.

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

GENERAL MEETING.

BULLETIN

OF THE

GENERAL MEETING.

227TH MEETING. JANUARY 13, 1883.

The President in the Chair.

Twenty-six members present.

Mr. H. FARQUHAR completed a communication begun at the 224th meeting on

EXPERIMENTS IN BINARY ARITHMETIC,

in which he showed that simple addition involved carrying on several distinct mental operations almost simultaneously and a capital of more than fifty propositions committed to memory. Believing that the difficulty in mastering, and the mental strain and liability to error in conducting, this most important of mathematical processes could be proved to be unnecessarily great, he had compared the time occupied in adding a few dozen numbers of six or eight figures each with that required when these numbers were expressed in powers of 2, the mental work being, in the latter case, reduced to counting similar marks and halving their sums. He had found it best to give different forms to the marks denoting neighboring powers, so as to avoid confusion of columns, and had combined two or more of them into one written figure for brevity of expression. About seventy combinations of various shapes had been tried, but very few of them found economical. In the best notation, however, the addition required only three-fourths the time taken with the ordinary figures. Had the computer practised as many weeks with the new notation as years with the old, the difference would have been much more marked; as it was in fact when one unskilled in arithmetic, to whom the binary notation had just been taught, tried the two additions. The gain in accuracy, with this observer, was even more striking than the gain in speed. There could be very little doubt, therefore, that a fair degree of skill in arithmetic with a binary notation could be acquired by many to whom it is impossible under the present system.

The only practicable division of arcs and angles, and the most natural division of all things, is by continued bisections. This is shown by the ratio of value in our coins, weights, and capacity measures; by any table of prices; and by the prevalent subdivision of lowest nominal units, as of the carpenter's inch into eighths and sixteenths, and of percentages into quarters, etc., in stock quotations, where convenience of calculation by our present arithmetic seems almost gratuitously sacrificed. The American coinage is inconvenient in practice, because of the awkward fractional ratio 2½, which it introduces between successive pieces; and there would be the same difficulty in a decimal system of weights or of measures, should it be imposed upon us. We have thus another powerful reason for endeavoring to introduce a binary arithmetic.

In the remarks which followed, Mr. E. B. ELLIOTT expressed the hope that Congress would adopt the metric system of weights and measures for international purposes. It would be better to secure what advantage could be gained from uniformity and consistency, even though the basis of consistency was an arithmetic not ideally the best attainable. Such a course would not prevent, but might pave the way for a better arithmetic.

Mr. W. B. TAYLOR said the world was losing so much by the employment of the denary arithmetic that he thought even a single generation might find economy in substituting the octonary. The introduction of decimal measures, while it would aid the computer, would injure the remainder of the community. The paper of Mr. Farquhar had an especial value, in that it proved the ability of binary systems to compete with the established system in rapidity of computation.

Other remarks were made by Messrs. HARKNESS, MUSSEY, POWELL, and GILBERT.

The next communication was by Mr. S. M. BURNETT on

REFRACTION IN THE PRINCIPAL MERIDIANS OF A TRIAXIAL ELLIPSOID; REGULAR ASTIGMATISM AND CYLINDRICAL LENSES;

and he was followed by Mr. W. HARKNESS on

THE MONOCHROMATIC ABERRATION OF THE HUMAN EYE IN APHAKIA.

These two papers are complementary, and are published in the *Archives of Ophthalmology*, Vol. XII, No. 1.

228TH MEETING. JANUARY 27, 1883.

The President in the Chair.

Thirty-seven members present.

The Auditing Committee, appointed at the Annual Meeting, reported through its chairman, Mr. Antisell, that it had examined the accounts of the Treasurer for 1882, and found them correct.

The report was accepted.

The communication of the evening was by Mr. H. H. BATES on

THE NATURE OF MATTER,

and was discussed by Mr. W. B. TAYLOR and Mr. POWELL.

This paper is published in the Popular Science Monthly for April, 1883.

229TH MEETING. FEBRUARY 10, 1883.

The President in the Chair.

Forty-two members and visitors present.

It was announced that reports of the scientific proceedings would hereafter be furnished to Science.

Mr. W. H. DALL announced that an opportunity would be afforded members to contribute to the Balfour Memorial Fund.

A communication was then read by Mr. A. F. A. KING on

THE PREVENTION OF MALARIAL DISEASES, ILLUSTRATING, *inter alia*, THE CONSERVATIVE FUNCTION OF AGUE.

[Abstract.]

The various theories thus far presented in explanation of the phenomena of malaria were unsatisfactory and insusceptible of scientific demonstration.

According to the best medical authorities the most generally admitted facts upon which the present orthodox theory of malaria rests were as follows: 1. Malaria affects by preference low and moist localities. 2. It is almost never developed at a lower temperature than 60° F. 3. Its evolution or active agency is checked by a temperature of 32° F. 4. It is most abundant and most virulent as we approach the equator and the sea-coast. 5. It has an affinity for dense foliage, which has the power of accumulating it, when lying in the course of winds blowing from malarious localities. 6. Forests or even woods have the power of obstructing and preventing its transmission under these circumstances. 7. By atmospheric currents it is capable of being transported to considerable distances—probably as far as five miles. 8. It may be developed in previously healthy places by turning up of the soil, as in making excavations for the foundations of houses, tracks for railroads, and beds for canals. 9. In certain countries it seems to be attracted and absorbed by bodies of water lying in the course of such winds as waft it from the miasmatic source. 10. Experience alone can enable us to decide as to the presence or absence of malaria in any given locality. 11. In proportion as countries, previously malarious, are cleared up and thickly settled, periodical fevers disappear, in many instances to be replaced by typhoid fever (?) 12. Malaria usually keeps near the surface of the earth. It is said to "hug the ground," or "love the ground." 13. It is most dangerous when the sun is down, and seems almost inert during the day. 14. The danger of exposure after sunset is greatly increased by the person exposed *sleeping* in the night air. 15. Of all human races the white is most sensitive to marsh fevers, the black least so. 16. In malarial districts the use of fire, both indoors and to those who sleep out, affords a comparative security against malarial disease. 17. The air of cities in some way renders the poison innocuous; for, though a malarial disease may be raging outside, it does not penetrate far into their interior. 18. Malarial diseases are most prevalent towards the latter part of summer and in the autumn. 19. Malaria is arrested not only by trees, but also by walls, fences, hills, rows of houses, canvas curtains, gauze veils, mosquito nets, and probably by fishing nets. 20. Malaria spares no age, but it affects infants much less frequently than adults.

These generally admitted facts were insusceptible of scientific explanation by the marsh fever hypothesis of Lanscisci; but were capable of explanation by the theory that marsh fevers are produced by the bites of proboscidian insects, notably in this and in some other countries by mosquito bites.

A review of the natural history, habits, and geographical distribution of the mosquito was next presented in explanation of the twenty statements above quoted.

In discussing statement 15, it was maintained that the comparative immunity of the black races was largely due to color, the dark complexion of the skin being another illustrative instance of "protective coloring" so often observed in other animals, and by which, in this instance, the negro was protected from the *sight*, and consequently from the *bite* of the mosquito; a similar protection being further secured by the offensive odor and greasiness of his cutaneous secretions, aided by artificial inunction of the body with grease, paint, pitch, &c., which last probably constituted the initial step in the evolution of dress. Hence malarial melanosis was considered to be the designed natural termination of ague—its conservative function—destined to modify the individual by defensive adaptation against the mosquito, whose penetrating proboscis, like an inoculating needle, infected the body with malarial poison, no matter whether this last was mosquital saliva, the *Bacillus malariæ* of Klebs and Crudelli, or some other element as yet unknown.

The spleen, whose function is not yet settled by physiologists, was regarded as the chief *pigment-forming organ*, and was *designed for this purpose in the economy of the organism.* Generally considered a superfluous organ, capable of removal without any great interference with the functions of the organism, it was naturally designed to *meet the emergency of variation in skin-color* to secure "protective coloring" against fever-producing proboscidian insects, as before indicated. The natural process, however, required exposure of the naked body to the sun during the chill stage, in order to secure deposit of the newly formed pigment in the skin. Nature had not anticipated the artificial appendage of dress, and the organism had not inherited from ancestral progenitors any provision for so unexpected an addition. Chills do not occur at night, but only between the rise and setting of the sun; sunlight during the chill stage being a necessary requirement, in order that nature's design of cutaneous chromatogenesis may be consummated. Other racial differences between the whites and blacks—such as even cerebral capacity and variations in the skeleton—might be susceptible of explanation by blood changes resulting from malaria. The marrow of bones was also a pigment-forming tissue, and the aching of bones during ague, especially in so-called "break-bone" fever, suggested congestion and modified nutrition in the osseous structures, such as might eventually lead to modification in the skeleton. The inhabitants of oriental countries especially were more vigorous and intelligent if they lived in elevated regions, than were others inhabiting mosquito-infected lowlands and sea coasts.

In further support of the mosquital origin of malarial fevers numerous noted medical authorities were cited, showing that, in all parts of the world where these diseases prevail, immunity was secured by protecting the body from mosquito bites. The geographical distribution and seasonal evolution of mosquitoes and other proboscidian insects were shown partially to agree with the times and places in which malarial diseases prevail; though from lack of information conclusive evidence on this point was yet wanting. There was, however, a general admission on the part of medical authorities that swarms of these insects in almost any locality were a pretty sure sign of malignancy.

On the other hand numerous instances were adduced from "Narratives" and "Travels" in which the bodies of persons had been covered with pustules, "resembling small-pox," from mosquito bites without any subsequent occurrence of fever having been *recorded* by the narrating authors.

This opposing evidence was inconclusive, (1) because the authors cited were not in search of medical information; (2) because the period of incubation, being often long and uncertain, fever *may* have occurred after the mosquito bites had been forgotten; (3) the insect proboscis (like a vaccine lancet unarmed with virus) might be uncontaminated with fever poison, or fever germs; and (4) *successful* inoculations of specific germ poisons are *not* usually followed by immediate local suppuration at the point of puncture, but only after a certain period of incubation, the *immediate* local inflammation being rather *preventive* of subsequent blood infection.

The possible spread of yellow-fever contagion by the inoculating proboscis of the mosquito carrying infecting matter drawn from the blood of yellow-fever patients to unaffected persons was suggested. In epidemics, the spread of the disease stopped as soon as a freezing temperature paralyzed the mosquito, &c.

The spread of spotted-fever, typhus-fever, in jails, ships, &c., was referred to the inoculating instrument of fleas, &c.—these insects usually prevailing among filthy people thickly crowded together.

That malarial diseases were ever produced solely by the *inhalation* of supposed poisonous vapors was held to be untenable. Experimenters, who had demonstrated the existence of specific poisons for special fevers, had equally proven that the mode by which such poisons, when obtained, could be introduced into the body for the artificial production of disease, was by *inoculation through the skin.* These experiments were imitations of insect inoculation. The proboscis of the mosquito was Nature's inoculating needle.

The *modus operandi* of the eucalyptus tree in preventing malarial diseases was ascribed tentatively to the tree being destructive to, or interfering directly or indirectly with, the propagation and development of mosquitoes.

From the foregoing conceptions as to the origin of malarial disease, the following prophylactic measures were deducible:

1st. *Personal* protection from all winged insects, especially during evening and night, by gauze curtains, veils, window-blinds, or clothing impenetrable by the proboscis of inoculating insects; and further, personal protection both from these and all creeping insects, especially during epidemics, endemics, and in crowded jails, ships, &c., by daily inunction of the whole body with some terebinthinate, camphorated, or eucalyptalized ointment or liniment.

2d. *Domiciliary* protection (*a*) *exteriorly*, by screens of trees, walls, fences, &c., interposed at some distance between dwellings and the supposed sources of malaria, or mosquito nurseries; and with fires or lamps arranged as traps for the attraction and destruction of such winged insects as may encroach nearer. A further protection (*b*) in the *interior* of dwellings being secured by the use of smoke (as of tobacco or prethrum) or of some volatile aromatic substance, as of camphor, assafœtida, garlic, &c., which may be offensive to proboscidian intruders.

3. *Municipal* protection by groves of trees (pines, cedars, or eucalyptus) planted between cities and the sources of malaria and mosquitoes, together with cordons of electric or other lights, between said grove and the marsh, the lights to be arranged as fly-traps for the retention and destruction of such winged insects as may be thus secured.

With relation to the city of Washington, it was suggested that the Washington monument would afford a good opportunity (by placing illuminated fly-traps at different elevations on its exterior) for ascertaining the height at which mosquitoes fly, or are brought by the wind from the adjacent Potomac flats. The proposed reclamation of the flats could scarcely do more than mitigate malarial disease, so long as our summer and autumn southern breezes come, laden with mosquitoes, from the miles of unreclaimed swamps farther down the river, as at Four-mile Run and other nearer localities.

Mr. BILLINGS remarked that, since ague did not invariably result from insect bites, the most that could be claimed was that they accomplished an accidental inoculation with malarial poison.

The subject was also discussed by Messrs. DOOLITTLE, TONER, and ANTISELL.

The meeting closed with an exhibition by Mr. C. E. DUTTON of a series of oil paintings illustrative of the Hawaiian Islands.

230TH MEETING. FEBRUARY 24, 1883.

Vice-President BILLINGS in the Chair.

Thirty members and visitors present.

The Chair announced the election of Mr. THOMAS RUSSELL to membership.

The first communication was by Mr. G. K. GILBERT on

THE RESPONSE OF TERRESTRIAL CLIMATE TO SECULAR VARIATIONS IN SOLAR RADIATION.

[Abstract.]

Secular variations of climate may theoretically be caused (1) by the internal heat of the earth and (2) by changes in the constitution or volume of the atmosphere. They have unquestionably been wrought (3) by changes in the limits and configuration of ocean bottoms and land surfaces, (4) by changes in the movements of the earth with reference to celestial bodies, and (5) by variations of solar radiation. Attention will here be restricted to the last-mentioned cause.

An augmentation of the strength of solar radiation (*a*) will cause a general rise in the temperature of the atmosphere, (*b*) will heighten the contrast between warm and cold regions, thereby stimulating oceanic and atmospheric circulation, and (*c*) will heighten the contrast between wet and dry regions, making the wet wetter and the dry drier. (*d*) It will also diminish glaciation. This has been disputed by some writers, but is sustained by a quantitative discussion. A computation, based on the annual curves of precipitation and temperature at St. Bernard, close to the glaciers of the Alps, shows that a general rise in the temperature of the air, while it will increase the total precipitation, will slightly diminish the snow-fall; that it will very greatly increase the rate of melting. The ratio of snow-fall to evaporation is reduced one-half by 6° C rise of temperature; the ratio of snow-fall to melting is reduced one-half by a rise of 1½°; and, assuming that evaporation actually dissipates twice as much snow as does melting, the ratio of snow-fall to snow dissipation (or the tendency to glaciation) is reduced one-half by 4½° rise of temperature.*

(*e*) Increase of solar radiation will also, through its general effects, influence the distribution of winds, and thus produce secondary effects of a local nature.

Mr. DALL remarked that ice was rendered more plastic and fluent by the presence of water; so that the movement of ice and the consequent extent of glaciers are favored by rain. If Mr. Gilbert by the term "glaciation" referred to the *extent* of glaciers, some limitation of his conclusions might be necessary.

Other remarks were made by Messrs. ANTISELL, DOOLITTLE, H. FARQUHAR, and ELLIOTT.

The next communication was by Mr. J. W. CHICKERING on

THE THERMAL BELTS OF NORTH CAROLINA.

[Abstract.]

In the agricultural volume of the Patent Office Report for 1861 is an article written by Mr. Silas McDowell, of Franklin, Macon county, N. C., bearing this title. He was a man of much intelli-

* The computation is given in full in "Science" for March 16, 1883.

gence, an enthusiastic student in geology and botany, a companion and guide of several botanists in their early explorations of the southern Appalachians, and a farmer by profession. He died in 1882, at the ripe old age of 87.

He states that in the valley of the Little Tennessee river, in Macon county, lying about 2,000 feet above tide water, when the thermometer in the morning indicates a temperature of about 26°, the frost line extends about 300 feet in vertical height, but that then comes a belt extending about 400 feet in vertical height up the mountain side, within which no frost is seen, delicate plants remaining untouched. Above this, frost again appears. So sharp is the dividing line that sometimes one-half of a shrub may be frost killed, while the other half is unaffected.

A small river, having its source in a high plateau 1,900 feet above this, runs down into this valley, breaking through three mountain barriers, and consequently making three short valleys, including the plateau, rising one above the other, each of which has its own vernal zone, traversing the hillsides that enclose it, and each beginning at a lesser elevation above the valley, as the valleys mount higher in the atmosphere, so that around the plateau, a beautiful level height, containing 6,000 acres of land, and lying 3,900 feet above tide water, the lower edge of the thermal belt is not more than 100 feet above the common level of the plateau.

Not only does vegetation within this zone remain untouched by frost, so that the Isabella, the most tender of all the native grapes, has not failed to produce abundant crops in twenty-six consecutive years, but mildew, blight, and rust, which often attack vines in the lower valleys, are here unknown, while the same purity and dryness of the air which favor the grape, make this a refuge for the consumptive, as diseases of the lungs have never been known to originate among the inhabitants.

Mr. McDowell adds: "The thermal belt must exist in all countries that are traversed by high mountains and deep valleys, and the only reason why its visible manifestations are peculiar to our southern Alleghanies, is the fact that their precocious spring vegetation is sometimes killed by frost, while the same thing does not happen in the mountains further north."

These statements are corroborated by similar testimony respecting another such belt along the Tryon mountain range in Polk county, N. C.; the specific claim being that such a belt is found for eight miles in length, extending from 1,200 feet to 2,200 feet above tide water, within which the leaves of plants, shrubs, and flowers remain untouched by frost until the latter part of December, and after a snow storm not a particle of snow remains within the belt, while the tops and sides of the mountains above and the valleys below will be covered.

The verification of these alleged facts would be matters of interest in their economical and sanitary aspects, and would supply data for some interesting researches respecting the nocturnal stratification of the atmosphere.

It is earnestly to be hoped that at some time we may have reliable and continuous thermometrical observations at these and similar stations, to determine the existence, extent, and temperature of such belts.

Remarks were made on this communication by Mr. ALVORD.

Mr. C. E. DUTTON then made a communication on the

GEOLOGY OF THE HAWAIIAN ISLANDS.

[Abstract.]

On the slopes of Mauna Loa are sea beaches, terraces, coral sands, and other evidences of shore action at various levels. The highest that can be positively announced has an altitude of 2,800 feet above the ocean. It can be traced a large part of the way around the island, being discernible even when covered by more recent lava. It does not now lie horizontal, but descends from 2,800 to 400 feet, while on the adjoining island, Maui, there is evidence of submergence. On the farther (western) side of Maui, and on other islands beyond, there is again evidence of upheaval.

All the lavas of the islands are basaltic. Those of Mauna Loa and Kilauea are abnormally basic and are related to certain lavas of New Zealand, called by Mr. Judd "ultra-basalts." The New Zealand rock consists chiefly of olivine; that of Mauna Loa is sometimes more than half olivine, and contains much magnetite and hematite. A Greenland lava, classed also as ultra-basalt, contains the only known native iron of telluric origin. As this suggests the iron meteorites, so the basalts of New Zealand and Mauna Loa suggest the stony meteorites.

The volume of the eruptions of Mauna Loa is enormous; that of 1855 would nearly build Vesuvius, and two of prehistoric date were greater still. The lava has a high liquidity and flows forty to fifty-five miles, spreading at the base of the cone into a broad sheet. There are no explosive phenomena and no fragmental products. The slope of the mountain is 4° along the major and 7° along the minor axis. Kilauea has a few cinder cones on its flanks. Mauna Kea consists chiefly of them, and has an average slope of 7½° to 11°.

Kilauea is always active, maintaining lakes of liquid fire. Over one of these a crust is formed, black, but flexible, which after a while breaks up and suddenly sinks, the process being repeated at intervals of 1½ to 2½ hours. The great interior pit described by observers from 1823 to 1841 is now filled.

Mauna Loa is not active more than one-third or one-fourth of the time, but compensates by the magnificence of its phenomena. Great fountains of lava are projected hundreds of feet into the air.

Mr. Dutton's communication was interrupted by the arrival of the hour for adjournment. In response to a question by Mr. TAYLOR, he stated that the crust over a lava lake acquired a thickness of five or six inches before breaking up.

Mr. ANTISELL inquired whether there is any basalt on the islands, and Mr. Dutton explained that they are composed exclusively of that material.

231ST MEETING. MARCH 10, 1883.

Vice-President WELLING in the Chair.

Thirty-four members and visitors present.

The Chair announced that Messrs. ALBERT WILLIAMS, Jr., JOHN HENRY RENSHAWE, and HENRY FRANCIS WALLING had been elected to membership.

Mr. M. H. DOOLITTLE read a communication on

SUBSTANCE, MATTER, MOTION, AND FORCE,

which was discussed by Messrs. W. B. TAYLOR, ELLIOTT, HARKNESS, and WELLING.

Mr. E. B. ELLIOTT then communicated

FORMULAS FOR THE COMPUTATION OF EASTER.

In the calendar the vernal equinox is considered as invariably occurring on the 21st of March.

The Paschal full moon is the full moon which (according to the calendar) occurs on or first after the 21st of March.

Easter Sunday in any year is the first Sunday which occurs *after* the Paschal full moon; that is, first after the full moon which, according to the calendar, occurs *on or first after* March 21st.

To find the date of Easter Sunday for any year, A. D., New Style.

Let c denote the complete hundreds of years in the number denoting any year, and y the number of remaining years. Thus in the year 1883, $c = 18$ and $y = 83$, the number for the entire year, 1883, being denoted by $100\,c + y$.

In the following formulas w, as a subscript after a division, denotes that only the whole number of the quotient is to be retained, and r, as a subscript, denotes that only the remainder after the division is to be retained; thus $\left(\frac{18}{4}\right)_{\text{w}} = 4$; and $\left(\frac{18}{4}\right)_{\text{r}} = 2.$

n (the golden number *less* one)

$$= \left(\frac{\text{year}}{19}\right)_{\text{r}} = \left(\frac{100\,c + y}{19}\right)_{\text{r}} = \left(\frac{5\,c + y}{19}\right)_{\text{r}} = \left(\frac{5\,c + y}{20 - 1}\right)_{\text{r}}$$

$$= \frac{1}{19}\left[5\left(\frac{c}{19}\right)_{\text{r}} + \left(\frac{y}{19}\right)_{\text{r}}\right]_{\text{r}}$$

This number (n) pertains to a lunar cycle of 19 years.

$$s = \overline{c - 8} - \left(\frac{c}{4}\right)_{\text{w}} - \left(\frac{c + 1 - \left(\frac{c+8}{25}\right)_{\text{w}}}{3}\right)_{\text{w}}$$

Inspection of the formula for s will show that, for any year from 1700 A. D. New Style to 1899 A. D., both inclusive, the value of s is zero (0). For any year Old Style the value of s is the constant number 22.

$$q = \left(\frac{23 + s + 19\,n}{30}\right)_{\text{r}}; \text{ also,}$$

$$= \left(\frac{23 + s - 11\,n}{30}\right)_{\text{r}}$$

$$h = \left(\frac{q}{29}\right)_{\text{w}} + \overline{29 - q}\left(\frac{q}{28}\right)_{\text{w}}\left(\frac{n}{11}\right)_{\text{w}}$$

The value of h may be shown to be zero (0) for any year from 1700 A. D. to 1899 A. D., both inclusive, during New Style, and for all years during Old Style.

$p = q - h =$ the interval in days from March 21st to the date of the Paschal full moon, or the number of days to be added to March 21st to find the date of the Paschal full moon.

If $p =$ zero (0), the Paschal full moon accordingly falls on the 21st of March.

$$L = \left(\frac{1 + 2\left(\frac{c}{4}\right)_r - y - \left(\frac{y}{4}\right)_w + \left(\frac{c}{40}\right)_w}{7} \right)_r$$

L denotes the number (in alphabetical order) of the Dominical or Sunday letter. Thus, the number corresponding to the Dominical letter A is 1, to B is 2, to C is 3, to D is 4, to E is 5, to F is 6, and to G is 7 or 0 (zero).

The term $\left(\frac{c}{40}\right)_w$ gives a correction to the Gregorian value when the year exceeds 4000 A. D.; for any year less than 4000 the value of this corrective term is obviously zero (0).

$$t - 1 = \left(\frac{3 - p + L}{7}\right)_r = \left(\frac{3 + 6p + L}{7}\right)_r$$

t denotes the number of days which elapse after the date of the Paschal full moon to the date of Easter Sunday.

$$\begin{aligned} \text{Easter Sunday} &= \text{March } (21 + 1 + p + \overline{t - 1}) \\ &= \text{March } (21 + p + t) \\ &= \text{April } (p + t - 10) \end{aligned}$$

To find the date of Easter Sunday for any year, A. D., Old Style.

$$n = \left(\frac{\text{year}}{19}\right)_r = \left(\frac{100\,c + y}{19}\right)_r$$

The formula for n is the same as in New Style.

$$q = p = \left(\frac{23 + 22 + 19\,n}{30}\right)_r = \left(\frac{15 + 19\,n}{30}\right)_r = \left(\frac{15 - 11\,n}{30}\right)_r$$

$$L = \left(\frac{3 + c - y - \left(\frac{y}{4}\right)_w}{7} \right)_r$$

$$t - 1 = \left(\frac{3 - p + L}{7}\right)_r = \left(\frac{3 + 6p + L}{7}\right)_r$$

$$\text{Easter Sunday} = \text{March } (21 + 1 + p + \overline{t - 1})$$
$$= \text{March } (21 + p + t)$$
$$= \text{April } (p + t - 10)$$

Example 1.—*Required the day of the month on which Easter Sunday falls in the year* 1883 *A. D., New Style.*

$$n = \left(\frac{5\,c + y}{19}\right)_{r} = 5\left(\frac{c}{19}\right)_{r} + \left(\frac{y}{19}\right)_{r}$$

$$\left(\frac{18}{19}\right)_{r} = 18 \text{ or } -1\,;\ 5\left(\frac{18}{19}\right)_{r} = 90 \text{ or } -5$$

$$\left(\frac{83}{19}\right)_{r} = \left(\frac{4 \times 19 + 7}{19}\right) = 7$$

$$n = -5 + 7 = 2$$

$$20\,n = 40$$

$$19\,n = 20\,n - n = 38$$

$$s = \overline{18 - 8} - \left(\frac{18}{4}\right)_{w} - \left(\frac{18 + 1 - \left(\frac{18 + 8}{25}\right)_{w}}{3}\right)_{w}$$

$$= 10 - 4 - \left(\frac{19 - 1}{3}\right)_{w} = 10 - 4 - 6 = 0$$

$$q = \left(\frac{23 + s + 19\,n}{30}\right)_{r} = \left(\frac{23 + 0 + 38}{30}\right)_{r} = 1$$

$$h = \left(\frac{1}{29}\right)_{w} + 29 - 1\left(\frac{1}{28}\right)_{w}\left(\frac{2}{11}\right)_{w} = 0 + 28 \times 0 \times 0 = 0 + 0 = 0$$

$$p = q - h = 1 - 0 = 1$$

$$L = \left(\frac{1 + 2\left(\frac{18}{4}\right)_{r} - 83 - \left(\frac{83}{4}\right)_{w} + \left(\frac{18}{40}\right)_{w}}{7}\right)_{r}$$

$$= \left(\frac{1 + \overline{2 \times 2} - 83 - 20 + 0}{7}\right)_{r} = \left(\frac{1 + 4 - 6 - 6}{7}\right)_{r} = 0$$

$$t - 1 = \left(\frac{3 - p + L}{7}\right)_{r} = \left(\frac{3 - 1 + 0}{7}\right)_{r} = 2$$

$$\text{Easter Sunday} = \text{March } (21 + 1 + p + \overline{t - 1})$$
$$= \text{March } (22 + 1 + 2)$$
$$= \text{March } 25$$

Example 2.—*Required the date of Easter Sunday for the year* 1884 *A. D., New Style.*

$$5\left(\frac{18}{19}\right)_r = -5$$

$$\left(\frac{84}{19}\right)_r = 8$$

$$n = 8 - 5 = 3$$

$$20\,n = 60$$

$$20 - n = 19\,n = 57$$

$$s = 0$$

$$q = \left(\frac{23 + 0 + 57}{30}\right)_r = 20$$

$$h = \left(\frac{20}{29}\right)_w + \overline{29 - 20}\left(\frac{20}{28}\right)_w\left(\frac{3}{11}\right)_w$$

$$= 0 + 9 \times 0 \times 0 = 0 + 0 = 0$$

$$p = q - h = 20 - 0 = 20$$

$$L = \left(\frac{1 + 2\left(\frac{18}{4}\right)_r - 84 - \left(\frac{84}{4}\right)_w + \left(\frac{18}{40}\right)_w}{7}\right)_r$$

$$= \left(\frac{1 + 4 - 0 - 0 + 0}{7}\right)_r = 5$$

$$t - 1 = \left(\frac{3 - 20 + 5}{7}\right)_r = \left(\frac{8 - 6}{7}\right)_r = 2$$

Easter Sunday = March (21 + 1 + 20 + 2)
= March 44
= April (44 — 31)
= April 13

Example 3.—*Required date of Easter Sunday for the year* 3966 *A. D., New Style.*

$$2 \times 19 = \begin{array}{c|c} 39 & 66 \\ \hline 38 & 57 \\ \hline 1 & 9 \end{array} = 3 \times 19$$

$$n = \left(\frac{5\,c + y}{19}\right)_r = \left(\frac{5 \times 1 + 9}{19}\right)_r = 14$$

$$20\,n = 280$$

$$19\,n = 20\,n - n = 266$$

$$s = \overline{39-8} - \left(\frac{39}{4}\right)_w - \left(\frac{\overline{39+1} - \left(\frac{39+8}{25}\right)_w}{3}\right)_w$$

$$= 31 - 9 - \left(\frac{40-1}{3}\right)_w = 31 - 9 - 13 = 9$$

$$q = \left(\frac{23 + s + 19\,n}{30}\right)_r = \left(\frac{23 + 9 + 266}{30}\right)_r = 28$$

$$= h \left(\frac{28}{29}\right)_w + \overline{29 - 28}\left(\frac{28}{28}\right)_w \left(\frac{14}{11}\right)_w$$

$$= 0 + 1 \times 1 \times 1 = 0 + 1 = 1$$

$$p = q - h = 28 - 1 = 27$$

$$L = \left(\frac{1 + 2\left(\frac{39}{4}\right)_r - 66 - \left(\frac{66}{4}\right)_w + \left(\frac{39}{40}\right)_w}{7}\right)_r$$

$$= \left(\frac{1 + \overline{2 \times 3} - 3 - 2 + 0}{7}\right)_r = 2$$

$$t - 1 = \left(\frac{3 - p + L}{7}\right)_r = \left(\frac{3 - 27 + 2}{7}\right)_r = 6$$

Easter Sunday = March (21 + 1 + 27 + 6)
= March 55
= April (55 − 31 =) 24

EXAMPLE 4.—*Required the date of the Paschal full moon (March* $\overline{21 + p}$*), and the date of Easter Sunday (March* $\overline{21 + p + t}$ *or March* $\overline{21 + 1 + p + (t - 1)}$ *for the year* 2152 *A. D., New Style.*

$$\left(\frac{2152}{19}\right)_r = 5 = n$$

$$19\,n = 95$$
$$s = 1$$
$$23 = 23$$

$$19\,n + s + 23 = 119$$

$$q = \left(\frac{19\,n + s + 23}{30}\right)_r = \left(\frac{119}{30}\right)_r = 29$$

$$h = 1 + \overline{0 \times 0 \times 0} = 1$$

$$p = q - h = 28$$

Paschal full moon = March (21 + 28 =) 49

= April (49 — 31 =) 18

$$L = \left(\frac{1 + 2\left(\frac{21}{4}\right)_r - 52 - \left(\frac{52}{4}\right)_w + \left(\frac{21}{40}\right)_w}{7} \right)_r$$

$$= \left(\frac{1 + \overline{2 \times 1} - 52 - 13 + 0}{7} \right)_r$$

$$= \left(\frac{1 + 2 + 0 - 3 - 6}{7} \right)_r = \left(\frac{3 - 2}{7} \right)_r = 1$$

$$t - 1 = \left(\frac{3 - 28 + 1}{7} \right)_r = 4$$

Easter Sunday = March (21 + 1 + 28 + 4 =) 54

= April (54 — 31 =) 23

The Julian or Old Style Calendar was established by the Council of Nice A. D. 325; the first year of the Gregorian or reformed calendar was A. D. 1582, and the first year in which the reformed calendar was adopted in England was A. D. 1752.

In Russia, and in other countries where the religion of the Greek Church now obtains, the New Style of reckoning has *not* been adopted, but the Old Style is still in force.

In Alaska, Old Style was employed until after the cession of that country by Russia to the United States in the year 1869.

EXAMPLE 5.—*Find the date of Easter Sunday for the year* 1582 *A. D., Old Style.*

$$\begin{array}{r|l}
15 & 82 \\
5 + 15 & = 75 \\
\hline
20 - 1) & 157 \quad (7 \\
 & 140 - 7 \\
\hline
 & 17 + 7 = 24 = 19 + 5 \\
 & n = 5
\end{array}$$

$$19n = (20 - 1)n = 100 - 5 = 95 = 3 \times 30 + 5$$

$$q = \left(\frac{19n + 15}{30} \right)_r = \left(\frac{5 + 15}{30} \right)_r = 20$$

$$L = \left(\frac{3 + 15 - 82 - 20}{7} \right)_r$$

$$= \left(\frac{18 - 5 - 6}{7} \right)_r = \left(\frac{18 - 11}{7} \right)_r = 0$$

$$t - 1 = \left(\frac{3 - 20 + 0}{7}\right)_r = 4$$

$$\text{Easter Sunday} = \text{March } (22 + 20 + 4 =) \, 46$$
$$= \text{April } (46 - 31 =) \, 15$$

232D MEETING. MARCH 24, 1883.

Vice-President WELLING in the Chair.

Forty-three members and visitors present.

The first communication was by Mr. J. R. EASTMAN on

THE FLORIDA EXPEDITION FOR OBSERVATION OF THE TRANSIT OF VENUS.

[Abstract.]

The observing station of the Florida expedition was upon Way Key, the largest of the group of islands known as Cedar Keys.

The principal instruments employed were a portable transit, a five-inch equatorial telescope, and a photoheliograph. The first two require no description. The photoheliograph consisted of an objective of five inches aperture and about forty feet focus, a heliostat for throwing the sun's rays on the objective, and a plate holder at the focus of the objective. The accessory apparatus consisted of a measuring rod, permanently mounted, for accurately measuring the distance from the objective to the photograph plate; a movable slide with a slit of adjustable width, for exposing the plates; and a circuit connecting with a chronograph, so arranged that when the exposing slide was moved to expose the plate, and when the center of the slit was opposite the center of the plate-holder, the circuit was broken and the record made on the chronograph. A black disk was painted on one side of the slide, and so placed that when the slide was at rest at one end of its course and the image of the sun was adjusted concentric with this disk, it would fall on the center of the plate-holder when the slide was moved. The adjustments having been completed the exposing of the plates was a simple matter. The image of the sun was thrown by the heliostat upon the black disk and centered, the sensitive plate was fixed in

the plate-holder, the operator moved the exposing slide, and the time of exposure was recorded on the chronograph.

For observing contacts I used an eye piece, magnifying 216 diameters, attached to a Herschel solar prism, and a sliding shade-glass with a density varying uniformly from end to end. The time of my signals was taken by assistant astronomer Lieut. J. A. Norris, U. S. N., from a chronometer; while, with an observing key, I also made a record on the chronograph as a check.

About 40 seconds before the computed time of first contact a narrow stratus cloud passed on to the southeastern edge of the sun and shut out all the light. The cloud remained about 3 minutes, and when it passed off, the notch in the sun's limb was plainly marked. Two photographs were taken to test the apparatus and the plates, and then the time before *second* contact was devoted to an examination of the limbs of Venus and the sun. Both were perfectly steady. In observations of the sun for the last twenty years I never saw it better. At about 13 minutes after first contact the outline of the entire disk of Venus could be seen, and seemed perfectly circular. About 2 minutes later a faint, thin rim of yellowish light appeared around the limb yet outside the sun. This rim was at first broadest near the sun's limb, but soon the width of the light became uniform throughout. The light was wholly exterior to the limb of Venus; that is, the black limb of Venus on the sun and the dark limb outside formed a perfectly circular disk, with the rim of light or halo, outside the portion off the sun. As the time of second contact approached, Lieutenant Norris again took up his station at the chronometer. As the limbs neared geometrical contact, the cusps of sunlight began to close around Venus more rapidly; and the perfect definition of the limbs and the steady, deliberate, but uniformly increasing motion of the cusps, convinced me instantly that the phenomena attending the contact would be far more simple than I had ever imagined. I had only to look steadily to see the cusps steadily but rapidly extend themselves into the thinnest visible thread of light around the following limb of Venus and remain there without a tremor or pulsation. At the moment the cusps joined I gave the signal and also made the record on the chronograph. Still keeping my eye at the telescope, I saw nothing to note save the gradually increasing line of light between the limbs of the two bodies. The disk of Venus on the sun was black.

A re-examination was then made of all the photographic apparatus, and about 10 minutes after the second contact the principal photographic work was commenced; and this was continued with slight interruption until about 10 minutes before third contact; 150 dry plates and 30 wet ones being exposed. One of the interruptions was for the purpose of making measurements of the diameter of Venus, which was done with a double-image micrometer attached to the 5-inch telescope.

On going to the telescope to observe the last contacts, I found the limbs of Venus and the sun as steady as in the morning, and though there was now some haze over the sun it did no harm. The third contact was observed with great accuracy, nothing occurring to obstruct or complicate the very simple and definite phenomena, which were in the reverse order of those seen at second contact. The rim of light appeared around Venus as soon as the limb was visible beyond the sun, and was seen for nearly 10 minutes. The complete outline of Venus was visible for 2 minutes longer. No phenomena worthy of note were seen between third and fourth contacts. The lapping of the limb of Venus over that of the sun gradually but steadily decreased until the final separation, which was observed with great accuracy for such a phenomenon. Soon after the last contact the entire apparatus was again carefully examined and the necessary observations made to determine the errors of the chronometers.

In the observations of interior contacts there was no trace of any tremor or fluctuation of the light in the cusps as they closed around the limb of Venus; and it is almost needless to say that there was no trace of a shadow or a black drop or ligament between the limbs at second and third contacts. The probable error for the second and third contacts was estimated at 0″.3; for fourth contact, 0″.5.

Observers of transits of Venus and Mercury have written so much in regard to the obstacles encountered from the apparition of the shadow, or black drop, between the limbs of the two bodies at *second* and *third* contacts, and so full has been the testimony in favor of the existence and the almost necessary occurrence of this phenomenon, that at the transit of Mercury, in 1878, many observers claimed, as evidence of their skill, that they *did* see it; while others, less fortunate, apologized for *not* seeing it. Observers of the black drop were so generally confined to those with imperfect apparatus or to those unaccustomed to observation of the sun's limb or disk that the true nature of the obstacle was pretty well understood before it was carefully investigated. It is now quite well settled that the "black drop" is due to bad eyes, imperfect apparatus, or the inexperience of the observer. With good eyes and proper apparatus a good observer never should see the black drop. When it is seen there is something wrong; it is a spurious phenomenon.

One of the negatives was exhibited to the Society.

In reply to a question by Mr. E. J. FARQUHAR, Mr. EASTMAN said the halo about Venus was believed to be due to the atmosphere of the planet.

The next communication was by Mr. CLEVELAND ABBE on

DETERMINING THE TEMPERATURE OF THE AIR.

He stated that the question now to be considered is not where to place a thermometer so as to obtain the temperature most proper for the use of the meteorologist, but is rather the purely physical question of how to determine the temperature of the air at any given location. He described the methods and defects of the former and present meteorological methods of exposure, viz: (1) Thermometers hung in the open air. (2) Those placed in shady locations. (3) The Glaisher screen. (4) The Stevenson screen and the double louvre screens in general. (5) The double metallic cylindrical shelters of Jelinek and Wild. (6) The silver thimble screen of Regnault. (7) The whirling thermometer of Saussure, Arago, Bravais, and the French observers (exhibiting Babinet's arrangement as made by Casella.) (8) Joule's method, depending on a balance in the temperature and density of two columns of the air.

He then gave a description of the method devised by him in 1865 and used for a short time at Poulkova; this consisted in constructing a very perfect louvre screen, within which were established black bulb and bright or silvered bulb thermometers having very diverse coefficients of radiation and conduction. These thermometers were in air, not *in vacuo*, as this latter arrangement was proper only for the determination of the direct solar radiation, as in the Arago-Davy method, whereas in the present case the temperature of the air and the radiation from terrestrial objects were the special objects of study.

The air temperature (t_a) was found from the indications of the bright and black bulbs (t_s and t_b) by the empirical formula

$$t_a = t_s + C\,(t_b - t_s)$$

where C is a small coefficient, to be determined experimentally, and is nearly constant. This arrangement of bright and black bulbs can be used by meteorologists and physicists without a screen, and even in the sunlight, if the theory of the action of the bright and black bulbs is perfectly understood. A similar formula will give the temperature (T) of a single radiating body whose effect is equal to the total effect that is shown by the black bulb:

$$T = t_b + C'\,(t_b - t_a.)$$

He then stated that the theoretical basis of this method has quite recently been further elucidated by Professor Ferrel, who has shown that the approximate nature of the relation between the above constant C, the radiating, absorbing, and conducting powers of the thermometers, and the velocity of the wind is given by the following equation:

$$C = \frac{1 + \dfrac{Br_b}{B' + B''v}}{\dfrac{r_b}{r_s} - 1}$$

where r_b and r_s are the radiating (and absorbing) powers of the blackened and silvered bulbs, respectively, v is the velocity of the wind or currents flowing past the bulbs, and B B′ B″ are constant coefficients depending on the size, conductivity, and specific heat of the substance of the bulbs.

In reply to a question of Mr. GILBERT, he stated that the difference between the bright and black bulbs had rarely exceeded a few tenths of a degree in the delicate shelter made of oiled paper, as used by him at Poulkova, the maximum occurring February 22, 1866, at 10 a. m., when, the louvre box being in the full sunshine, the bright bulb was at 14°.9 Cent. and the black bulb at 14°.3, showing that the latter had been slightly warmed by the warm sides of the box.

In reply to a question of Mr. HARKNESS, the author explained, that although it was conducive to accuracy that these thermometers should be placed within a shelter, yet this was not necessary; if we take advantage of the more accurate method of determining the co-efficient constant C, as given by Prof. Ferrel's theory, the two thermometers placed anywhere within doors or without would still give data for determining temperatures of the location; it should be borne in mind that the temperature thus obtained belongs specifically to the air in contact with the themometers and is not an average value for any extensive portion of the atmosphere. As it is an advantage to conduct observations under uniform conditions, it is recommended that a pair of bright and black bulb thermometers be attached to the whirling table, whereby the effect of a current of air may be on the one hand determined and on the other hand kept as uniform as possible.

Mr. HARKNESS said that the object practically sought by meteorologists was to learn the average temperature of a considerable body of air, but their efforts were thwarted by the irregularity and inconstancy of the distribution of temperature. So long as the air in contact with the thermometer is not precisely representative of the air of the vicinage it was useless to refine methods of observation, unless by that refinement errors of a constant nature were eliminated. For the determination of mean monthly or annual temperatures he considered the reading of the nearest half degree as sufficient, and regarded the reading of the tenths of a degree as a useless refinement.

The advantage of reading to tenths was further discussed by Messrs. ABBE, DOOLITTLE, and KUMMELL. Mr. KUMMELL pointed out that where a difference of temperature is observed as an indication of the moisture of the air, the tenths are worthy of record.

The following communication by Prof. CHARLES E. MUNROE, of Annapolis, Md., was then read by the Secretary:

DETERMINATION OF THE SPECIFIC GRAVITY OF SOLIDS BY THE COMMON HYDROMETER.

Having occasion some time since to devise methods for the examination of coal on board ship, I was obliged, as my first consideration, to work with such materials and apparatus as are usually found in ships' stores, and then to arrange the methods so that they could be used under the restricted conditions which prevail. The unsteadiness of the ship makes balance methods for the determination of specific gravities difficult, even when a suitable balance is at hand, while hydrometers may be steadied so that the instrument may be read with a reasonable degree of precision, as is shown in its constant use in the determination of the degree of saturation of the water in the steam-boiler, and in other instances.

To use the hydrometer for the determination of the specific gravities of solids I take advantage of the fact that, when a body floats in a liquid in which it is wholly immersed, the specific gravities of the liquid and the solid are the same, and we have simply to determine the value for one of them.

The process is carried out by taking a dense solution, dropping in it the solid to be determined, (which must be light enough to float on the surface,) and then diluting slowly with water until the solid floats immersed, stirring the mixture constantly. The solid is now removed and the hydrometer inserted and read. For the determination of the specific gravities of the bituminous coals and lignites a thick solution of cane sugar was used, while for the heavier anthracite concentrated sulphuric acid, diluted with dilute sulphuric acid, was employed. The increase in temperature in the latter case causes no appreciable error if the reading is quickly taken. The following results were obtained by the method described, the specific gravity of each specimen having first been determined by Jolly's balance:

	By Jolly's balance.	By mixture.
Anthracite	1.5640	1,560
Bituminous coal	1,3008	1,310
Bituminous coal	1,3000	1,300
Gas coal	1,2790	1,285
Cannel coal (ligniform)	1,1550	1,155
Cannel coal	1,1292	1,120
Lignite	1,0909	1,090

Mr. DUTTON remarked that the same principle had recently been successfully applied to the separation of the component minerals of crystalline rocks. A sample is powdered and then placed in a very heavy liquid (a solution of mercuric iodide and potassium iodide), the density of which is gradually diminished, until the particles of the heaviest mineral sink to the bottom. A repetition of the process eliminates each mineral in turn.

233D MEETING. APRIL 7, 1883.

MR. WM. H. DALL in the Chair.

Thirty-six members and visitors present.

The Chair announced that Messrs. EDWARD SANDFORD BURGESS and SUMNER HOMER BODFISH had been elected members.

The General Committee reported to the Society that "a Mathematical Section had been organized by the election of Mr. ASAPH HALL as Chairman and Mr. HENRY FARQUHAR as Secretary. All members of the Society who are interested in mathematics are invited to attend and take part in its meetings, announcements of which will be sent to those who notify the Secretary of a desire for them."

The first communication was by Prof. W. C. KERR on

THE GEOLOGY OF HATTERAS AND THE NEIGHBORING COAST.

[Abstract.]

The notable projection of Hatteras, beyond the general line of trend of the Atlantic coast, has, of course, a geological origin. The study of the changes now taking place, and of the phenomena which have left their recent traces on the surface, readily furnish the data for the solution of the problem. Nearly one-half of this eastern inter-sound region of North Carolina is water surface, and the land surface lies for the most part below ten feet (much of it below five.)

A large part of this low-lying surface is covered with beds of peat, which thicken towards the centre on the divides or swells between the bays and sounds, rising, in some cases, to ten and fifteen feet, and in the Dismal Swamp on the northern border of the State to twenty-two feet. These beds of peat are in process of forming by the decay of plants growing on the surface, chiefly cypress and juniper. Many tiers of the undecayed logs of these timbers are piled upon one another through the whole thickness of the deposit, which is soft and yielding, so that a fence-rail may be thrust down beyond its length. Vast tracts of such peat swamps (and of marsh and savanna on which only water grasses and small shrubs and scrub pines grow and decay) are found throughout this coast region. Here we have the first stage in the formation of a coal bed. Another notable fact is that many of the rivers which empty into the sounds increase in depth of channel at a distance from their mouths; while the sounds are 12 to 15 and 20 to 22 feet deep, the rivers are often 30 and 40 feet and upwards. This can only be accounted for by supposing a subsidence of the region to be in progress, the sounds and open bays being silted up by the deposits brought down by the floods of the Roanoake and other large rivers, while no particle of sediment can reach the sheltered depths of the narrow windings of the upper reaches of these minor streams. This theory of subsidence is abundantly confirmed by the disappearance under water of large tracts of swamp bordering the rivers, as the Chowan, within the observation of men now living, and by the existence of rooted stumps of cypress and juniper in the bottom of the bays and sounds, even to the depth of 15 and 20 feet, and also by the vertical and crumbling shores of the sounds, undermined and eroded by the advancing waves.

The Atlantic ocean is walled off from this region by a narrow fringe of sand islands, or dunes, blown shoreward by the wind and thrown up into reefs and hillocks like snow-drifts 50, 80, and even more than 100 feet high. The movement of these sand waves being inland, the sounds are silting up next the sea, and are in many places converted into marshes 3 to 5 miles wide. The reef is increasing in continuity and breadth, most of the inlets above Hatteras that were open 300 years ago being closed and obliterated. An inspection of the form of the curves of the submarine contours off Hatteras and adjoining coasts will show that the action of the tides and ocean currents, the Gulf stream and Arctic current meeting at this point, accumulate upon Hatteras the river silt which reaches the sea by way of the Chesapeake as well as that of the rivers which discharge their burdens through the inlets about this point and southwards. Which amounts to this—that Hatteras may be described as a sort of *delta*, whose materials are derived from the drainage of more than 100,000 square miles of the Atlantic slope.

A subsidence of about 20 feet would bring the sea again over the entire Sound region and carry the shore 75 miles inland, bringing Hatteras to coincide with Cape Lookout. A sand reef, like that north of Hatteras, marks the line of the ancient shore, when these conditions obtained. A depression of fifty feet would move the shore 100 miles west of Hatteras and carry the point of meeting of the conflicting ocean currents and waves to Cape Fear. A subsidence of 500 feet, as in the glacial period, would carry

Hatteras more than 200 miles west of its present position. This horizon is marked by an immense sand reef, still retaining its wind and wave marks, and rising to a height of more than 500 feet above tide, the reef itself being at least 100 feet deep and many miles in length. The sea must have remained at this level for a very long period.

But Hatteras is not a modern phenomenon. It is at least as old as the cretaceous; the quaternary as well as the tertiary of this coast region of North Carolina are laid down upon an eroded surface of cretaceous rock, while the artesian borings, at Charleston, reach this formation at 700 feet, and at the mouth of the Chesapeake they do not seem to have touched it at 1,000 feet.

Mr. WARD remarked that, in traversing the Jericho canal of the Dismal Swamp in a row boat, he had observed an outward flow at both ends of the canal, showing that, by continuous water passage, a divide was crossed between Lake Drummond and the James river.

He criticised the doctrine taught in text-books and popular writings that the preservation of leaves in a fossil state is due ordinarily to river action and delta formation. More favorable conditions are to be found in swamps.

Other remarks were made by Messrs. DUTTON and HOUGH.

The second communication was by Mr. H. F. WALLING on

TOPOGRAPHICAL INDICATIONS OF A FAULT NEAR HARPER'S FERRY.

[Abstract.]

A description was given of a break in the continuity of the Blue Ridge, where its disconnected portions, extending side by side for a few miles, are cut by the Potomac river, near Harper's Ferry, the gorges so formed presenting a striking feature of the scenery.

The two ridges, here about 12,000 feet apart, stretch for hundreds of miles in nearly parallel directions, one to the south and the other to the north; the latter being known in Pennsylvania as the South Mountain. The strike of the rocks is parallel to the ridges, about N. 30° E., and the prevailing dip is eastward, averaging not more than 30°. The ridges are composed of hard sand-rock; the adjacent region, of lime-stone and other rocks more easily disintegrated or dissolved.

Supposing the sand-rock of the Blue Ridge and South Mountains to have been originally a continuous formation, it will be readily seen that a vertical fault in easterly dipping strata, having its direction somewhat nearer the meridian than the present strike and its downthrow on the west side of the fault, would produce a lateral discontinuity like that here observed, the upthrown part of any stratum cropping out on the east of the downthrown part at a distance depending upon the amount of the vertical displacement.

All this would depend upon whether the sand rocks were originally continuous in the two ridges—a question which was left for the geologists to decide. The writer, however, took occasion to suggest that great longitudinal faults might be formed near coast lines when the gradual overloading of the balanced crust by depositions of sediment produced a strain too great to be relieved by flexure. A rupture would then occur, the strata going down on the overloaded side of the fault and up on the other until equilibrium of pressure upon the yielding magma below was restored by lateral displacement of the magma. The fault so formed would present a diminished resistance to dislocation, and if the action which originated it should continue, it would be likely to increase in dimensions both in length and in the amount of vertical displacement. This action might even continue after the emergence of the region above the surface of the water, provided a more rapid denudation of the landward than of the seaward side of the fault took place, in which case a continued disturbance of equilibrium would be accompanied by vertical yielding, increasing the amount of dislocation, and by subterranean movements of the supporting magma, whereby a restoration of material would be effected from overloaded to denuded areas.

Moreover, the hypothesis of a constant restoration of disturbed equilibrium makes it easier to understand why the folding of strata should grow steeper, even to a *folding under*, as the axis of a mountain chain is approached. A diagram exhibiting the so-called "fan-like structure of the Alps," enlarged from a figure by Rogers, (see Rogers' Report on the Geology of Pennsylvania, Vol. II, p. 902,) was shown in illustration. The gradual subterranean movements inward under a mountain chain, as the upper portions were removed and the remainder elevated, would carry the strata along on a support of diminishing width until they were folded upward and backward.

The gradual increase towards the east in the amount of corrugation and steepness of dips, together with the supposed reversed folding by which the rocks of the eastern part of the Appalachian region seem to dip under older rocks, still further east appear, therefore, to favor the notion that the paleozoic rocks of the Appalachian region and the eastern part of the Mississippi basin were derived from the erosion of highlands formerly existing east of the Appalachian chain, now, perhaps, submerged in the Atlantic ocean. The downthrow of a fault, if formed in the manner supposed in the region under consideration, would accordingly be on its western side, as suggested above.

The third communication was by Mr. S. F. EMMONS on

ORE DEPOSITION BY REPLACEMENT.

[Abstract.]

After a few introductory remarks upon the relatively unsatisfactory condition of that branch of geology which treats of ore deposits, considering the early date at which it was taken up, the speaker briefly reviews the existing theories and classifications, and shows that they are mainly based on the idea that each ore deposit is the filling of some pre-existing cavity or opening in the rock in which it is now found; that so-called fissure veins, for instance, were once actually open cracks, and that irregular deposits in limestone have been made by the filling up of open caves, such as so frequently occur in these rocks. The result of his studies of the so-called "carbonate deposits" of Leadville, Colorado, has been to show that they are *not* the filling up of pre-existing cavities; the caves there have been formed since the ore was deposited, as is proved by their crossing indiscriminately ore bodies and limestone. They belong to a class of deposits for which he proposes the name *metamorphic deposits*, or those which have been formed by a metasomatic interchange between the vein and original rock material. In Leadville the principal deposits are an actual replacement of the limestone itself at or near the contact of this stratum with an overlying sheet of porphyry. This replacement action has in places proceeded so far that the entire stratum of ore-bearing limestone or dolomite, originally 150 to 200 feet thick, has been changed into vein material, which consists of silica and metallic minerals. This vein material was brought in solution by percolating waters, which had taken it up during their circulation through the adjoining and generally overlying eruptive rocks. A more detailed description of the phenomena of these deposits will be found in his paper entitled "Abstract of a Report on the Geology of Leadville," in the Second Annual Report of the Director of the United States Geological Survey.

While the speaker's studies have thus far been mainly confined to limestone deposits, he has reason to believe that essentially the same process has produced a large proportion of ore deposits in crystalline and eruptive rocks, and that to the class of metamorphic deposits belong most of the so-called fissure veins of the Rocky Mountain region. That is, that they are not the filling in of pre-existent open fissures by vein materials foreign to the adjoining rocks, but simply a metamorphic change of these rocks themselves along channels of easy access to percolating waters; and according to the character of the material held in solution by these waters, these rocks have been more or less changed into quartz and metallic minerals, to a greater or less width, as the case may be. Numerous instances of such veins will be found in the forthcoming Census Report upon the Statistics and Technology of the Precious Metals, by Mr. G. F. Becker and the speaker.

234TH MEETING. APRIL 21, 1883.

Vice-President BILLINGS in the Chair.

Forty members present.

The Chair announced that Messrs. WASHINGTON CARRUTHERS KERR and SAMUEL FRANKLIN EMMONS had been elected members.

Mr. W. H. DALL addressed the Society on

GLACIATION IN ALASKA,

illustrating his remarks by maps of the territory and of the glacial areas of the St. Elias Alps and Kachekmak Bay, Cook's Inlet, the latter being from surveys made by him under the direction of the U. S. Coast Survey.

He called attention in the first place to the wide differences in the character of the masses of ice resulting from the consolidation of snow by gravity (which would usually be classed as glaciers), as observed by him during nine years' exploration in Alaska.

These might be classed under several heads: as plateau-ice, filling large areas of depression and without motion as a whole, but when sufficiently accumulated overflowing the edges of its basin in various directions; as valley-ice, filling wide valleys of gentle incline both as to their axes and their lateral slopes, producing masses of ice moving in a definite direction but without lateral and sometimes even without terminal moraines; as ice-cascades, formed in sharp narrow ravines of very steep inclination, usually without well-defined surface moraines; as typical glaciers, showing névé and lateral and terminal moraines; and lastly, as effete or fossil glaciers, whose sources have become exhausted, whose motion has therefore ceased, and whose lower portions have become smothered by the accumulation of non-conducting débris. The very existence of one of these last has remained unknown for half a century, though the plateau underwhich it is buried has been described and mapped by explorers.

Another form under which ice appears in Alaska is that of solid motionless layers, sometimes of great thickness, interstratified with sand, clay, etc. A deposit probably of this character is described by Nordenskiöld, on the Asiatic coast, near Bering Strait. In Alaska this formation, in which ice plays the part of a stratified rock, extends from Kotzebue sound, where the greatest known thickness of the ice-layer, about three hundred feet, has been noted, around the Arctic coast, probably to the eastern boundary. In Kotzebue Sound the ice is surmounted by about forty feet of clay containing the remains of fossil horses, buffaloes (*Bos latifrons*, etc.), mountain sheep, and other mammals. Farther north the ice is covered with a much thinner coat of mineral matter or soil, usually not exceeding two or three feet in thickness, and rarely rises more than twelve or fifteen feet above high water mark on the sea coast. Its continuity is broken between Kotzebue Sound and Icy Cape by rocky hills composed chiefly of carboniferous limestones, which bear no glaciers and do not seem to have been glaciated. The absence of bowlders and erratics over all this area has been noted by Franklin, Beechey, and all others who have explored it. The remarkable extent and character of the formation was unknown previous to the speaker's investigations, though the ice cliffs of Kotzebue Sound had attracted attention from the time of their first discovery.

Mr. Dall desired especially to emphasize the distinction between these strata of pure ice and the "frozen soil" so often alluded to by arctic explorers. The absence of frozen soil in the alluvium of the Yukon Valley, far north of Kotzebue Sound, was noted, as well as the fact that this valley has, for some unexplained reason, a mean temperature considerably above the normal, so that its forests extend well beyond the Arctic circle.

The distribution of glaciers, properly so-called, in Alaska, as far as our present knowledge goes, is confined to the region of the Alaskan range and the ranges parallel with it south of the Yukon Valley, but particularly to the coast mountains bordering on the Gulf of Alaska and the Alexander Archipelago, of which the Saint Elias Alps form the most conspicuous uplift.

The distribution of stratified ice is all north of the Yukon Valley, which divides the two regions. Hence, for the glacial epoch, it may be presumed that the one is the equivalent of the other, and the fact that Arctic Alaska is marked by stratified ice, rather than glaciers such as those of Greenland, must be due to local geological and climatic peculiarities existing at the time. On the Asiatic coast, especially at Holy Cross Bay, in nearly the same latitude and with not very different topographic conditions, glaciers are abundant at the present time.

On the mainland, facing the Alexander Archipelago, especially toward Lynn Canal, Icy Strait and the Stikine region, local glaciers are abundant, and traces of others, now dissolved, may be found on the lowlands of most of the islands. That these were always local, though doubtless very extensive, and that they were the progeny of the topography instead of being its parent, is obvious to anyone who has seen the coasts of Maine or Norway, which have been submitted to general glaciation, and will compare their rounded, worn, and *moutonnée* aspect with that of the sharp cliffs, beetling crags, narrow valleys, and scanty lowlands of the Alaskan islands.

The speaker concluded, from his observations, that the extent of the Alaskan glaciers is greatly diminished from its former state, and is probably still diminishing; that the southern portion of the Territory is probably nearly or quite stationary, while the northern part is undergoing elevation; and that, from the nature of the case, the area of stratified ice cannot be expected to increase or diminish materially without changes in geological or climatic conditions too great to be anticipated.

Mr. Alvord remarked that on Point Barrow frozen ground had been penetrated to a depth of thirteen feet.

In reply to a question by Mr. ANTISELL, Mr. DALL said that little was known of the humidity of the interior of Alaska; 23 inches of precipitation, nearly all in snow, had been observed in a single year at one point and 12 inches at another.

Mr. F. B. HOUGH then read a paper on

THE CULTIVATION OF THE EUCALYPTUS ON THE ROMAN CAMPAGNA,

which was discussed by Messrs. E. B. ELLIOTT and H. FARQUHAR. It is published in the American Journal of Forestry for June, 1883.

235TH MEETING. MAY 5, 1883.

Vice-President BILLINGS in the Chair.

Twenty-seven members and visitors present.

The Chair announced the election to membership of Messrs. WILLIAM THOMAS SAMPSON, JOHN OSCAR SKINNER, and THOMAS CROWDER CHAMBERLIN.

The first communication was by Mr. H. A. HAZEN on

HYGROMETRIC OBSERVATIONS.

[Abstract.]

After describing the various devices by which the moisture of the air has been measured, and especially the novel and valuable apparatus of Crova, the speaker illustrated the difficulty of the subject by contrasting synchronous determinations made at four points within a radius of two miles, and then described some experiments tending to show the inaccuracy of the wet and dry bulb hygrometer, as ordinarily observed. The value of the wet bulb reading is enhanced by blowing on the bulb with a bellows, or otherwise subjecting it to a brisk current of air.

Mr. HARKNESS remarked first, that Mr. Hazen's experiments appeared to prove the insufficiency of Regnault's formula, for they showed the difference between the indications of the wet bulb and dry bulb to be a function not only of the humidity, but of the velocity of wind; second, that height of station above the ground was a condition to which too little attention had been given; and third, that there seemed a possibility of obtaining a slightly erroneous vapor tension with Crova's apparatus.

Mr. E. J. Farquhar then read a paper on

DREAMS IN THEIR RELATION WITH PSYCHOLOGY.

[Abstract.]

Several theories of dreams were considered and none found entirely sufficient; not because a new and complete one was to be proposed, but because all seemed a little too partial and limiting in their scope. After touching on the relation of dreams to sleep and to waking, as intermediate between them, discrediting many recorded experiments on the ground of their being vitiated by a special purpose latent in the mind, and pointing out that the usual supposition of our being often waked by the intensity of a dream appears to put cause for effect, since it must be the fact of waking that effects the dream, perhaps by slow degrees—the character of mental operations in dreams was discussed. Dissent was expressed from the opinion that the dreaming state is devoid of such originating power as belongs to the waking; this position was maintained by showing first, the extreme vividness and lastingness of impression often pertaining to dreams, apart from any features of horror; then the coherence, far from being unknown among them, yet of a peculiar kind; and, finally, the true significance occasionally appearing in them, generally by figurative shape, amounting sometimes to a real enlightenment of the mind. Regarding the faculties or aspects of mind most apt to display themselves in dreams, it was held that all were liable to the exercise in turn, though some of the higher ones, especially the moral sense and judgment, less than others; since these expressed a rarer and more distinctive force evolved and laid up by and for our relations with actual life, while other powers whose exercise is less of an expenditure from the most important vitalities of mind were freer at the time—the principles of conservation and struggle for existence being thought to apply among the mental elements. Thus, to a certain degree, the mind may be seen more clearly in its true character by means of dreams than awake, though in very partial views at a time. Unconscious mental action was reviewed in this connection, and it was held that not only the lower processes, called reflex, but many of the highest functions

largely partake of this attribute. A great number of other points in regard to dreams were merely named as illustrating the fertility of the subject.

236TH MEETING. MAY 19, 1883.

Vice-President HILGARD in the Chair.

Forty members and visitors present.

It was announced from the General Committee that the following rules had been adopted:

I. If the author of any paper read before a section of the Society desires its publication, either in full or by abstract, it shall be referred to a committee, to be appointed as the section may determine.

The report of this committee shall be forwarded to the Publication Committee by the secretary of the section, together with any action of the section taken thereon.

II. Any paper read before a section may be repeated, either entire or by abstract, before a general meeting of the Society, if such repetition is recommended by the General Committee of the Society.

Mr. ROBERT FLETCHER made a communication entitled

RECENT EXPERIMENTS ON SERPENT VENOM.

It is published in the *American Journal of the Medical Sciences* for July, 1883.

Mr. H. FARQUHAR then made a communication on

FURTHER EXPERIMENTS IN BINARY ARITHMETIC,

showing that the relation between the vertical and horizontal dimensions of the characters used in the binary notation is a factor in determining its economic value. He presented, also, the results of a series of comparative tests showing that the binary notation enables some persons, after brief practice, to perform addition more rapidly than with denary notation, while with others it requires a longer time. The latter class includes practiced computers, generally, and the former those less accustomed to the use of figures.

Mr. DOOLITTLE remarked that the most instructive results would be obtained by experimenting with young persons; and the subject was further discussed by Messrs. W. B. TAYLOR, E. B. ELLIOTT, and C. A. SCHOTT.

237TH MEETING. JUNE 2, 1883.

Vice-President HILGARD, and afterward Mr. HARKNESS, in the Chair.

Twenty-two members present.

It was announced that the next meeting would be held October 13th.

Mr. W. LEE made a communication, with illustrations, entitled

SKETCHES FROM MEDALLIC MEDICAL HISTORY.

[Abstract.]

The paper was prefaced by remarks on the value of coin and medal collecting as a profitable means of instruction, and by a recognition of the danger to which collectors are exposed of developing a mania for collecting odd and curious things which cease to be instructive. An extended interest in numismatics commenced to show itself in this country in 1858, at which time there were probably not as many as one hundred coin collectors in the United States. The interest has grown rapidly, however, until now there must be on the books of the United States Mint the names of at least one thousand collectors who receive yearly the issue of the mint, with a special proof polish. In New York alone, during the year 1882, there were thirty-nine collections sold at public auction, the amount realized being $68,441.36. The largest of these was the Bushnell collection, which realized $13,900.47. Several of our large cities have numismatic societies, some of which are designated as numismatic and archæological societies; and a number of periodicals devoted simply to the interest of numismatics obtain a satisfactory circulation.

The modes of striking off coins and medals were given somewhat in detail, and attention was then called to the important part which medals struck in honor of medical men and to commemorate important events bearing directly upon the history of medicine have played throughout the history of the world. The illustrations of the paper included a hundred and fifty examples of the medals themselves, in regular sequence, from the days of Roman and Greek medicine down almost to the date of the paper itself, an interesting commemoration of events and individuals marking epochs in the history of medicine. These medals were taken up *seriatim*, references were made to the lives of individuals and the scientific work done by them, and descriptions were given of the occasions which called for the striking of medals.

The paper closed with an expression of hope that the Society might be stimulated at the sight of so many handsome and permanent memorials of the men and times of the past, to attempt to preserve the features of its first president, Joseph Henry, in a similar enduring form.

The bibliography of the subject was discussed at some length, and the following works were referred to:

MEAD, Richardi.—Dissertatio de Nummis quibusdam a Smyrnaeis in medicorum honorem percussis. Naples, 1752.

RUDOLPHI, C. A.—Index numismatum in virorum de rebus medicis vel physicis meritorum memoriam percussorum. Berlin, 1st edition 1823, 2d edition 1825, 12mo., XII, 131 pp, 3d edition 1828, 4th edition 1829. (This work (2d edition) comprises the description of 523 medals struck in honor of 350 scientific and medical men.)

RENAULDIN, Leop. Jos.—Études historiques et critiques sur les Médicins Numismatistes, contenant leur biographie et l'analyse de leurs écrits. Paris, 1851, 8°, XVI, 574 pp. (This work contains the names of 61 physicians).

CHÉREAU (A).—Les mereaux et les getons de l'ancienne faculté de médecine de Paris. L'Union Médicale. Paris, 1873, 3 Series, XV, pp. 309, 321.

PFEIFFER, (L) und RULAND (C).—Pestilentia in Nummis. Geschichte der grossen Volkskrankheiten in numismatischen Documenten. Ein beitrag zur Geschichte der Medicin und der Cultur. Tubingen, 1882, 8° X, 189 pp. Mit zwei Tafeln Abbildungen in lichtdruck.

WROTH, Warwick.—Asklepios and the Coins of Pergamon. From the Numismatic Chronicle and Journal of the Numismatic Society. London, 1882, Part I, Third Series, No. 5, pages 1 to 51, plates 3.

MOEHSEN, J. C. G.—The exact title of this author's work is not known to the writer of the paper; it was written in German,

and embodies a description of a collection of medals in Berlin struck in honor of physicians, giving 200 medals struck after the 15th century.

GROTEFEND, C. L.—Die Stempel der Römischen Augenärzte. Hannover, 1867.

Mr. T. N. GILL then made a communication on

ANALOGUES IN ZOO-GEOGRAPHY.

238TH MEETING. OCTOBER 13, 1883.

The Society, in accordance with the notice of adjournment at the June meeting, resumed its sessions.

The President in the Chair.

Forty-four members and visitors present.

It was announced that during the vacation the Society had lost by death Surgeon General C. H. CRANE, one of its Vice-Presidents; Admiral B. F. SANDS, one of its founders; and Dr. JOSIAH CURTIS.

It was further announced from the General Committee that Mr. GARRICK MALLERY had been appointed Vice-President to fill the vacancy occasioned by the death of Mr. CRANE, and that Mr. C. V. RILEY had been added to the General Committee to complete its number.

Mr. WILLIAM B. TAYLOR read a paper entitled

NOTE ON THE RINGS OF SATURN.

[Abstract.]

After an historic sketch of the varying and apparently incongruous observations by astronomers on the markings and aspects of the Saturnian rings, down to those of Schiaparelli of the Milan Observatory, (published in June last,) Mr. TAYLOR remarked that since the mathematical discussion by Prof. J. Clerk Maxwell, in 1857,* both the rigid and the fluid ring theories have been abandoned; and the discrete or meteoric constitution of the rings is now accepted by all physical astronomers as conclusively established.

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* *On the Stability of the Motion of Saturn's Rings.* 4to. 71 pp. and 1 plate. Cambridge, Eng., 1859.

Reference was then made to the startling announcement by Otto Struve, in 1851, that a careful comparison of the earlier with the later measurements showed that during the two hundred years of observation the rings had been widening, and the inner edge steadily approaching the body of the planet.* Considering the necessarily vast antiquity of the Saturnian system, such a change during the brief interval of human existence seems *à priori* almost infinitely improbable. The hypothesis of some that a meteoric ring has been drawn in by Saturn's attraction, within comparatively recent ages, seems entirely negatived by the circular symmetry of the system. It is not surprising, therefore, that Struve's inference has been received with an almost universal incredulity by the astronomical world. Robert Main, of the Greenwich Observatory, from a discussion of his own measurements taken in the winter of 1852–'3, and in 1854, disputed the accuracy of Struve's measures; and concluded that "no change has taken place in the system since the time of Huyghens."† And Prof. F. Kaiser, in a paper on "The Hypothesis of Otto Struve respecting the gradual increase of Saturn's Ring," etc., arrives at the same conclusion, and believes "there exists no reason whatever for supposing that the compound ring of Saturn is gradually increasing in breadth." ‡

There seems to be little doubt of some unintentional exaggeration in Struve's tabulated results, which range from 4″.6 : 6″.5 for the ratio of ring breadth to space between ring and ball, in the time of Huyghens, 1657, to 7″.4 : 3″.7 for the ratio of breadth to space, by his own observation in 1851. Nevertheless it is a noteworthy fact that all the early drawings of Saturn made in the seventeenth century (many of which are figured by Huyghens in his *Systema Saturnium*, 1659) plainly exhibit the width of the ring as sensibly less than the dark space within; while all modern observers would agree that the bright ring is now wider than the dark space, in about the ratio of 3 : 2; or were we to take the average of the esti-

* *Recueil des Mémoires présentés* [etc.] *par les Astronomes de Poulkova.* 4to. St. Petersburg, 1853. Vol. I, pp. 349–385. "Sur les Dimensions des Anneaux de Saturne." (Memoir read before Acad. Sci.) A brief abstract of the memoir is given in the *Monthly Notices, R. A. S.*, November 12, 1852. Vol. XIII, pp. 22–24.

† *Monthly Notices, R. A. S.*, December 14, 1855. Vol. XVI, pp. 30–36.

‡ *Mem. Acad. Sci., Amsterdam*, 1858. A translation of the memoir is given in the *Monthly Notices, R. A. S.*, January 11, 1856. Vol. XVI, pp. 66–72.

mates of the last century, it would probably not vary far from 5″.25 : 5″.75; while the general average for the present century would probably be about 6″.5 : 4″.5. There seems, therefore, to be a real difference, not accounted for by inferiority of earlier instruments and estimates, nor by the existing uncertainties of modern measurements. The question will probably be definitely settled in less than a century. Meanwhile there is a need of some explanation of the apparently systematic and progressive divergence first pointed out by Struve; and we naturally ask, What indications are afforded by theory?

The elder Herschel, in 1789, (at the Saturnian equinox, when the edge of the ring was presented to view,) from supposed observation of protuberances moving on the line, believed that he had detected a rotation, whose period he estimated at 10h. 32m. 15s., for the outer edge of the ring.* The correctness of this interpretation was controverted by Schroeter, from observations at Lilienthal, on the next passage of Saturn's equatorial node in 1803; as it was afterward questioned by Prof. G. P. Bond, of Harvard Observatory, from observations in 1848.† It is scarcely doubtful that Herschel's period was derived from an entire misconception of the nature of the ring—which he firmly held to be solid—and that it possesses no scientific value whatever. A. Secchi, from certain recurrent irregularities of phase observed at Rome in 1854, 1855, and 1856, inferred a rotation period of 14h. 23m. This is doubtless a nearer approximation (for the outer edge of the ring) than Herschel's estimate. It is not probable, however, that the period of any portion of the ring will be determined by observation.

Accepting the meteoric theory of the rings as now established, we may by Kepler's law compute with confidence the period of rotation of any part of the ring; and we thus find—

From the period of the inner Satellite (*Mimas*)	22h.	37½m.—
The period of outer edge of ring	14h.	30 m.
" dividing stripe	11h.	20 m.
" inner edge of bright ring	7h.	12 m.
" inner edge of dusky ring	5h.	45 m.
Mean period of ring (supposed solid) about	10h.	50 m.

The period of the planet Saturn is . . . 10h. 14m.

* *Phil. Trans. Roy. Soc.* 1790: Vol. LXXX, p. 479; and 1792: Vol. LXXXII, p. 6.

† Gould's *Astronomical Journal.* 1850. Vol. I, pp. 20, 21.

Thus regarding each constituent element of the ring as having its own independent rotation, (a condition absolutely essential to the stability of the system,) we may consider that from the complicated and variable perturbations by the exterior satellites, no one particle can revolve in a circular orbit, and hence that in a space so crowded there must be a considerable amount of interference. The collisions at intersecting orbits may result in heat or in disintegration; but in any event they must tend to a degradation of motion, and hence to a slightly shortened mean radius-vector and a shortened period.

Theoretically then such an effect as that indicated by Struve would seem inevitable, whether as a matter of fact it has been sufficient in a couple of centuries to be detected or not. And this involves a modified conception as to the earlier condition of the Saturnian rings. To suppose a fine web of nebulous matter continuously spun out from Saturn's equator, with an unchanging balance of centrifugal and centripetal forces during the long ages while the planet was slowly contracting to one-half its radius, is certainly no easy task or plausible theory. If, however, we are now beholding but a stage of transitional development of the ring, we shall have to imagine its primitive radius considerably larger, and its width as probably very much narrower—so narrow indeed as to have a planetary or satellitic status, revolving in a single definite period—possibly that of *Mimas* the nearest satellite. Such a ring would present a condition of comparatively great stability; and it may have been that only the secular recurrence of rare and remarkable conjunctions commenced upon it the work of disturbance and disintegration.

When Galileo, the first to see the strange appendages to Saturn, (though without being able to distinguish the *ansæ* as parts of a ring,) observed, in 1612, that they had entirely disappeared, he wrote in some dismay, "Has Saturn possibly devoured his own children?" * So may perhaps the future astronomer, seeing but an airy trace of the historic ring, repeat the saying, Saturn has indeed devoured his offspring; not indeed completely, for a part will probably still remain; nor with violent catastrophe, for the scattered fragments falling by their eccentricity will be absorbed as gently as are the meteors daily falling on our earth.

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* Third letter to Marc Velser, December 1, 1612. *Opere di Galileo.* 4to. 4 vols. Padua, 1744: Vol. II, p. 123.

A subsidiary point deserving of notice is the certainty that the inner portions of the bright ring (and still more those of the dusky ring) are revolving in periods three or four hours shorter than that of Saturn himself. When Professor Hall made his brilliant discovery of the satellites of Mars, and announced that the inner satellite (*Phobos*) was found to have the short period of 7h. 38m. (or less than one-third of that of Mars) the fact was at once proclaimed by some as incompatible with the "nebular hypothesis." Everybody knows that the rotation periods of the sun and planets do not conform to the third law of Kepler. Our own moon has an actual velocity in its orbit more than double that of our terrestrial equator. And had the moon a little less than one-third its present distance, (that is, were its radius-vector less than 70,000 miles,) its *angular* velocity would exceed that of the earth, or its period would be less than 24 hours. Or, stated in another way, our earth, if expanded to the orbit of the moon, (under the most favorable disposition of form and of homogenous density,) would occupy considerably more than a year in completing its rotation. The supposed nebular difficulty is therefore just as pertinent to our own satellite as to those of Saturn or of Mars. The obvious solution is, that all the planets (without exception) have lost a very large amount of rotatory energy; and this may be largely or chiefly ascribed to the retarding effects of internal friction resulting from solar tides. And, given time enough, the rotation of every planet should be finally reduced to the lunar condition of a precise accord of its diurnal and annual periods. On any hypothesis whatever, it is certain that the rotations of the planets are very much slower (notwithstanding too the acceleration due to contraction) than they originally were. This fact certainly offers no objection to the nebular hypothesis.

Mr. Dutton questioned the validity of Ennis' hypothesis, that the rotation of a nebular mass could be initiated by purely internal movements.

Other remarks were made by Mr. Frisby.

Mr. S. M. Burnett then made a communication on

THE CHARACTER OF THE FOCAL LINES IN ASTIGMATISM,

showing that the two lines which limit the focal interval of Sturm have been erroneously assumed to be straight. There is only one special case of the triaxial ellipsoid in which they are straight. In all other cases they are curved.

The full text of this paper may be found in the *Archives of Ophthalmology*, Vol. XII, Nos. 3 and 4.

Mr. H. A. HAZEN followed with a communication on

THERMOMETER EXPOSURE.

[Abstract.]

Without entering upon the question, Where in any locality shall the air temperature be observed, it is proposed to discuss the even more important question, What shall be the environment of a thermometer that it may give the true temperature. The practice has been very various: in England the Stevenson shelter is regarded as a standard: this is a double-louvred frame, wholly of wood, 18 x 10 x 18 inches, and placed about 4 feet above grass. In Russia we find a large wooden outside shelter of single louvres open to the north, inside of which is placed a metallic screen, the whole being exposed 12 or 13 feet above grass. In any exposure we should seek, first, to allow the freest possible access of the outer air, and second, to screen the thermometer from direct sun heat, from precipitation, and from radiation, whether (*a*) from surrounding objects by day or (*b*) to the sky at night.

It is important that we adopt some ready means of accurately determining the air temperature which may answer as a standard of comparison. This we have in the swung thermometer, which, by its free motion through a large body of air shaded from direct sunlight in the daytime, is calculated to give good results.

Experiments have been tried with a so-called "Pattern" shelter constructed of wood, of single louvres, inclined 30° to the horizontal, thus giving a good air circulation. The size is 4 x 3 x 3 feet, and it is erected at a height of 13 feet above a tin roof. In order to determine the least admissible size for a shelter, thermometers were placed in the Pattern 5 inches apart and running in an east and west direction, and these were observed morning and afternoon. It has been found that with a hot sun and still air the heat from the louvres rapidly diminishes with distance and becomes insensible at 15 inches. Comparisons have also been made for several weeks between the Russian and Pattern shelters; and the means of 100 sets of continuous observations on a still day, and again on a windy day, are shown n the following table:

	Dry thermometer.		Wet thermometer.		Relative humidity; per cent.	
	Russian.	Pattern.	R.	P.	R..	P
Still air	74°.8	73°.5	64°.0	62°.7	52.4	51.1
Light south wind	77 .2	77 .1	62 .0	61 .0	36.7	34.1

These results show directly the advantage of a good circulation of air, and that after shielding from the sun and radiation to the sky with a shelter at least 3 feet long, we may neglect other considerations.

Experiments are still in progress to determine the proper height above sod or roof, the proper exposure for a north window, and so forth.

Mr. ANTISELL, referring to the general theme rather than to the special subject of the paper, took occasion to note that the practice of conducting meteorologic observations on the tops of high houses, while it may well subserve the special purposes of the Signal Service, renders their work of materially less value to the medical profession. There is so much change, especially of the moisture element, in the first few feet from the ground upward that no observations can be depended upon as reporting the conditions of the phenomena of disease unless they are made in the layer actually occupied by man.

Mr. TAYLOR asked whether there might not be an error arising from the set given to the glass of the bulb by the pressure of the mercury of a whirled thermometer.

Mr. HAZEN replied that he had tested the effect of pressure applied to the bulb with the finger, and found that the set produced was of very brief duration. He had also tested the thermic effect of the friction on the atmosphere incurred by rapid whirling, and found it inappreciable with a velocity of about fourteen miles an hour. On whirling a black bulb thermometer, he observed a change of several tenths of a degree, which appeared clearly referable to the greater coefficient of friction of the surface roughened by lampblack.

Mr. GRAHAM BELL remarked that if we eliminate radiation and learn the absolute temperature of the air at the point of observation, our knowledge is still limited to that point only, whereas for meteorologic purposes it is important to ascertain the average temperature of a body of air. He suggested the possibility of utilizing for this purpose a measurement of the velocity of sound, which velocity is dependent on atmospheric temperature and independent of barometric pressure.

Mr. DUTTON thought that the extreme delicacy of this observation would involve an uncertainty greater than the one which now inheres in the determination.

239TH MEETING. OCTOBER 27, 1883.

The President in the Chair.

Forty-seven members and guests present.

The Chair announced the death of two members since the last meeting—LEONARD DUNNELL GALE and ELISHA FOOTE.

Announcement was also made of the election to membership of CHARLES DOOLITTLE WALCOTT.

Mr. T. N. GILL made a communication on

ICHTHYOLOGICAL RESULTS OF THE VOYAGE OF THE ALBATROSS.

Mr. ALEXANDER GRAHAM BELL made the following communication on

FALLACIES CONCERNING THE DEAF, AND THE INFLUENCE OF SUCH FALLACIES IN PREVENTING THE AMELIORATION OF THEIR CONDITION.

It is difficult to form an adequate conception of the prevalence of deafness in the community. There is hardly a man in the country who has not in his circle of friends and acquaintances at least one deaf person with whom he finds it difficult to converse excepting by means of a hearing-tube or trumpet. Now is it not an extraordinary fact that these deaf friends are nearly all adults? Where are the little children who are similarly afflicted? Have any of us seen a child with a hearing-tube or trumpet? If not, why not? The fact is that very young children who are hard of hearing, or who cannot hear at all, do not naturally speak, and this fact has given origin to the term "deaf-mute," by which it is customary to designate a person who is deaf from childhood.

"But are there no deaf children," you may ask, "excepting those whom we term deaf-mutes?" No; none. In the tenth census of the United States (1880) persons who became deaf under the age of sixteen years were returned as "deaf and dumb." Such facts as these give support to the fallacy that deafness, unaccompanied by any other natural defect, is confined to adult life, and is specially characteristic of advancing old age.

So constant is the association of defective speech with defective hearing in childhood that if one of your children whom you have left at home, hearing perfectly and talking perfectly, should, from some accident, lose his hearing, he would also naturally lose his speech. Why is this, and why are those who are born deaf always also dumb?

Fallacies Concerning the Dumbness of Deaf Children.

The most ingenious and fallacious arguments have been advanced in explanation. George Sibscota,* in 1670, claimed that the nerves of the tongue and larynx were connected with the nerves of the ear, "and from this Communion of the vessels proceeds the sympathy between the Ear, the Tongue and Larynx, and the very affection of those parts are easily communicated one with the other. Hence it is that the pulling of the Membrane of the Ear causeth a dry Cough in the party; and that is the reason most deaf men * * * are Dumb, or else speak with great difficulty; that is, are not capable of framing true words or of articulate pronunciation by reason, of the want of that convenient influx of the animal spirits; and for this cause also, it is that those who are thick of Hearing have a kind of hoarce speech."

The value of Sibscota's reasoning may be judged of by the further information he gives us concerning the uses of the Eustachian tube. "By this it is," he says, "that Smoakers, puffing up their Bheeks, having taken in the fume of Tobacco, send it out at their Ears. Therefore the opinion of *Alcmaeon* is not ridiculous, who held that she-Goats did breathe thorough their Ears," &c., &c.

It is easy for us to laugh at the fallacies of the past, but are we ourselves any less liable to error on that account? The majority of people at the present day believe that those who are born deaf are also dumb *because of defective vocal organs.* Now let us examine

* I have been informed that Sibscota's work, "The Deaf and Dumb Man's Discourse," from which the above extracts are taken, is in reality a translation of another work by Anthony Densing, published in 1656.

this proposition. It is a more ridiculous and absurd fallacy than that of Sibscota and more easily disposed of.

The hypothesis that congenitally deaf children do not naturally speak because their vocal organs are defective involves the assumption that were their vocal organs perfect such children *would* naturally speak. But why should they speak a language they have never heard? Do we speak any language that we have not heard? Are our vocal organs defective because we do not talk Chinese? It is a fallacy. The deaf have as perfect vocal organs as our own, and do not naturally speak because they do not hear. I have myself examined the vocal organs of more than 400 deaf-mutes without discovering any other peculiarities than those to be found among hearing and speaking children. The deaf children of Italy and Germany are almost universally taught to speak, and why should we not teach ours? Wherever determined efforts have been made in this country success has followed and articulation schools have been established.

Fallacy Concerning the Intelligence of Deaf Children.

The use of the word "mute" engenders another fallacy concerning the mental condition of deaf children. There are two classes of persons who do not naturally speak—those who are dumb on account of defective hearing and those who are dumb on account of defective minds. All idiots are dumb.

Deaf children are gathered into institutions and schools that have been established for their benefit away from the general observvation of the public, and even in adult life they hold themselves aloof from hearing people; while idiots and feeble-minded persons are not so generally withdrawn from their families. Hence the greater number of "mutes" who are accessible to public observation are dumb on account of defective minds, and not of defective hearing. No wonder, therefore, that the two classes are often confounded together. It is the hard task of every principal of an institution for the deaf and dumb to turn idiots and feeble-minded children away from his school—children who hear perfectly, but cannot speak. Although it is evidently fallacious to argue that, because all deaf infants are dumb, and all idiots are dumb; therefore all deaf infants are idiots: still this kind of reasoning is unconsciously indulged in by a large proportion of our population; and the majority of those who for the first time visit an institution for the deaf and dumb express unfeigned astonishment at the brightness and intelligence displayed by the pupils.

Why Hearing Children who become Deaf also become Dumb.

I have stated above that children who are born deaf do not naturally speak because they cannot hear. For the same reason children who lose their hearing after having learned to speak naturally tend to lose their speech. They acquired speech through the ear by imitating the utterances of their friends and relatives, and when they become deaf they gradually forget the true pronunciation of the words they know, and have naturally no means of learning the pronunciation of new words; hence their speech tends to become more and more defective until they finally cease to use spoken words at all.

Adults who become deaf do not usually have defective speech, for in their case the habit of speaking has been so fully formed that the mere practice of the vocal organs in talking to friends prevents loss of distinctness. We can learn, however, from the case of Alexander Selkirk how important is constant practice of the vocal organs. This man, after about one year's solitary residence upon an island, was found to have nearly forgotten his mother tongue; and we find that deaf adults who shrink from society and use their vocal organs only on rare occasions acquire peculiarities of utterance that are characteristic of persons in their condition, although the general intelligibility of their speech is not affected.

Fallacies Regarding the Nature of Speech.

The fallacies I have already alluded to respecting the difference between those who become deaf in childhood and those who become deaf in adult life have their origin in a fallacy concerning the nature of speech itself. To most people, who do not reflect upon the subject, it appears that speech is acquired by a natural process similar to that by which we acquire our teeth. At a certain age the teeth make their appearance, and at another age we begin to talk. To unreflecting minds it appears that we *grow into speech;* that speech is a natural product of the vocal organs, produced without instruction and education ; and this leads directly to the fallacy that where speech is wanting or imperfect the vocal organs are defective.

I have already stated that this cause has been assigned in explanation of the dumbness of children who are deaf. The idea gives rise also to the popular notion that stammering and other defects of speech are diseases to be "cured," and the attempt has been made to do so, even by heroic treatment. It is not so very long ago that slices have been cut from the tongue of a stammerer, in the vain hope of "curing" what was, after all, but a bad habit of speech. I have myself known of cases where the uvula has been excised to correct the same defect. The dumbness of the deaf and the defective speech of the hearing are some of the penalties we pay for acquiring speech ignorantly, by mere imitation. If parents realized that stammering and other defects of speech were caused by ignorance of the actions of the vocal organs, and not necessarily through any defect of the mouth, they would have their children taught the use of the vocal organs by articulation teachers, instead of patronizing the widely-advertised specialty physicians, who pretend by secret means to "cure" what is not a disease. Speech is naturally acquired by imitation, and through the same agency defects of speech are propagated. A child copies the defective utterance of his father.. A school-fellow mocks a stammering companion, and becomes himself similarly affected. In the one case the fallacy that the supposed disease is hereditary prevents attempts at instruction and correction, and in the other the idea that the affliction is the judgment of God in the way of punishment discourages the afflicted person and renders him utterly hopeless of any escape excepting by a miracle.

A practical illustration of the fact that defective speech is propagated by imitation is shown in my own case. When I was a boy my father was a teacher of elocution, and had living with him at one time one or two pupils who stammered. While under the care of my father, these boys spoke clearly and well, without any apparent defect, but, owing to his being called away for a protracted period of time, his pupils relapsed, and the boys commenced to stammer as badly as at first. Upon my father's return he found a house full of stammerers. *His own sons were stammering too!* I can well remember the process of instruction through which I went before the defect was corrected in my own case.

Ignorance the Real Difficulty in the Way of Teaching Deaf Children to Speak.

Speech is the mechanical result of certain adjustments of the vocal organs, and if we can teach deaf children the correct adjustments of the perfect organs they possess, they will speak. The difficulty lies with us. We learn to speak by imitating the sounds we hear, in utter ignorance of the action of the organs that accompanies the sounds. I find myself addressing an audience composed of scientific men, including many of the most eminent persons in the country, and I wonder how many there are in this room who could give an intelligible account of the movements of their vocal organs in uttering the simplest sentence? We must study the mechanism of speech, and when we know what are the correct adjustments of the organs concerned, ingenuity and skill will find the means of teaching *perfect* articulation to the deaf.

The Old Fallacy—" Without Speech, no Reason."

I have already stated that children who are born deaf are also always dumb. How, then, can they think? It is difficult for us to realize the possibility of a train of thought being carried on without words; but what words can a deaf child know, who has never heard the sounds of speech?

When we think, we think in words, though we may not actually utter sounds. Let us eliminate from our consciousness the train of words, and what remains? I do not venture to answer the question; but it is this, and this alone, that belongs to the thoughts of a deaf child.

It is hardly to be wondered at, therefore, that the fallacy should have arisen in the past that there could be no thought without speech; and this fallacy prevented for hundreds of years any attempt at the education of the deaf. Before the end of the last century deaf-mutes were classed among the idiots and insane; they had no civil rights, could hold no property; they were irresponsible beings. Even those interested in the religious welfare of the world consigned their souls to the wrong place, for "faith comes by hearing," and how could a deaf child be saved? I say that for hundreds of years the old fallacy, that "without speech there could be no reason," hindered and prevented any attempt at the amelioration of the condition of the deaf. But, strange to say, it was this very fallacy that first led to their education. It was attempted, by a miracle to teach them to speak.

In Bede's History of the Anglo-Saxon church we read "How Bishopp John cured a dumme man with blessing him."

"And when one weeke of Lent was past, the next sounday he willed the poore man to come unto him; when he was come, he bydd him put out his tounge and show it unto him, and taking him by the chinne, made the signe of the holy crosse upon his tounge, and when he had so signed and blessed it, he commauuded him to plucke it in again, and speake saying, speake, me one word, say *gea, gea,* which in the english tounge is a worde of affirmation and consent in such signification as yea, yea.* Incontinent the stringes of his tounge were loosed, and he said that which was commanded him to say. The bishopp added certain letters by name, and bid him say A; he said A; say B, he said B, and when he had said and recited after the bishopp the whole cross rewe he put upon him sillables and hole wordes to be pronounced. Unto which when he answered in all pointes orderly, he commaunded him to speake long sentences, and so he did; and ceased not all that day and night following, so longe as he could hold up his head from sleepe (as they make report that were present) to speake and declare his secret thoughtes and purposes, which before that day he could never utter to any man."†

Now, stripped of the miraculous, this is simply a case of articulation teaching. In the other countries of Europe the first attempts at the education of the deaf were also made by teaching them to speak, and as the early teachers were monks of the Roman Catholic Church, it is probable that these schools resulted from the attempts to perform the miracle of healing the dumb. A large proportion of the deaf and dumb who were thus brought together were successfully taught to articulate.

But now comes a marvel: It was found by the old monks that their pupils came to understand the utterances of others by watching the mouth. Such a statement appears more marvelous to those who understand the mechanism of speech than to those who are ignorant of it; and there is a general tendency to consider this accomplishment as among the fictitious embellishments of the old narratives. But the experience of modern teachers confirms the fact. John Bulwer, who is said to have been the earliest English writer upon the subject of the instruction of the deaf and dumb, published

* It will be remembered that the original of this was in Latin, and that "the english tounge" here means what we now call the Anglo Saxon.

† American Annals of the Deaf and Dumb, vol. I, p. 33 (1848).

in the year 1648 a treatise entitled "Philocophus; or, the Deaf and Dumbe Man's Friend. Exhibiting the Philosophicall verity of that subtile Art, which may inable one with an *observant Eie*, to *Heare* what any man speaks by the moving of his lips. Upon the same Ground, with the advantage of an Historicall Exemplification, apparently proving, That a Man Borne Deafe and Dumbe may be taught to *Heare* the sound of *words* with his *Eie*, and thence learn to speak with his tongue."

Articulation Teaching in America.

In Europe at the present time deaf children are much more commonly taught to speak and understand speech than in this country.

In the majority of our schools and institutions articulation and speech-reading are taught to only a favored few, and in these schools no use is made of articulation as a means of communication. A considerable number of the deaf children in our institutions could once hear and speak, and those pupils who retain some knowledge of spoken language have their vocal organs exercised for an hour or so a day in an articulation class under a special articulation teacher, but this is not enough exercise to retain the speech. I have seen a boy who became deaf at 12 years of age, and who had previously attended one of our public schools, go into an institution for the deaf and dumb talking as readily as you or I *and come out a deaf mute.*

Few, if any, attempts are made to teach articulation to those who have not naturally spoken, except at the special request of parents who desire that the experiment shall be tried with their children.

I have seen a congenital deaf mute, who also had a sister deaf and dumb, who was taught to speak in adult life, and I found upon experiment that he could understand by ear the words and sentences that he had been taught to articulate when they were spoken in an ordinary tone of voice about a foot behind his head, yet this young man had been educated at one of our best institutions without acquiring articulation, and as a consequence he grew up a deaf mute and married a deaf mute. He informed me himself that he could hear the people talking in the workshop where he was employed, but did not understand what they said.

As a matter of personal observation I am convinced that a large proportion of the congenitally deaf are only hard of hearing, and this belief is supported by the fact that it used to be the custom in some of our institutions to summon the pupils from the play-ground *by the ringing of a bell!* Does this not indicate that a large number of the pupils could hear the ringing of the bell, and that they told the others who could not hear at all? Such pupils could have been taught to speak at home by their friends if artificial assistance had been given to their hearing. There was no necessity for their ever becoming deaf and dumb.

It is only within the last fifteen years or thereabouts that schools have been established in the United States where all the deaf children admitted are taught articulation and speech-reading, but such schools are rapidly increasing in number. Still, it is not generally known that the experimental stage has passed, and that all deaf mutes can be taught intelligible speech. This is now done in Italy and Germany, and the international conventions of teachers of the deaf and dumb held recently at Milan and Brussels have decided in favor of articulation for the deaf.

I have stated before that the difficulties in the way of teaching articulation are external to the deaf. They lie with us and in our general ignorance of the mechanism of speech. A teacher who does not himself understand the mechanism of speech is hardly competent to produce the best results. So dense is the general ignorance upon this subject that it is probable that of the 50,000,000 of people in this country the number of persons who are familiar with all that is known concerning the mechanism of speech might be numbered on the two hands. Considering this, the success obtained in our articulation schools is gratifying and wonderful.

Upon the Art of Understanding Speech by the Eye.

It has been found in the articulation schools of this country that deaf children can acquire the art of understanding by eye the utterances of their friends and relatives, and this fact has led some teachers to suppose that speech is as clearly visible to the eye as it is to the ear, and this fallacy tends to hinder the acquisition of the art by their pupils.

When we examine the visibility of the elementary sounds of our language we find that the majority can not be clearly distinguished by the eye. How then, you may ask, can a deaf child who cannot distinguish the elements understand words which are combinations of these elements?

When the lips are closed we cannot see what is going on inside the mouth. The elementary sounds of our language, represented by the letters P, B, and M, involve a closure of the lips. Hence the differences of adjustment that originate the differences of sound are interior and cannot be seen. But while the deaf child may not be able to say definitely whether the sound you utter is P, B, or M, he knows certainly that it must be one of these three, for no other sounds involve a closure of the lips. And so with the other elements of our language. While he may not be able to tell definitely the particular element to which you give utterance, he can generally refer it to a group of sounds that present the same appearance to the eye. In the same manner he may not be able to tell the precise word that you utter, but he can refer it to a group of words having the same appearance. For instance, the words "pat," "bat," and "mat" have the same appearance to the eye. While he cannot tell which of these words you mean when it is uttered singly, he readily distinguishes it in a sentence by the context. For instance, were you to say that you had wiped your feet upon a "mat," the word could not be "pat" and it could not be "bat."

Here we come to the key to the art of understanding speech by the eye—Context. But this involves, as a prerequisite, a competent knowledge of the English language; and we may particularly distinguish those children who have acquired the art from those who have not, by their superior attainments in this respect. We can, therefore, see why children who have become deaf after having learned to speak, naturally acquire this power to a greater extent than those who are born deaf.

There are many cases of congenitally deaf children who have acquired this art as perfectly as those who have become deaf from disease; but in every case such children have been thoroughly familiar with the English language, at least in its written form.

Fallacies Regarding Speech-reading.

The fallacy that speech is as clearly visible to the eye as it is audible to the ear hinders the acquisition of the art by causing the teacher to articulate slowly and word by word, even opening the mouth to its widest extent to make the actions of the organs more visible. When we realize that context is the key to speech-reading, theory asserts that ordinary conversational speech should be more intelligible than slow and labored articulation. This is amply proved by the experience of the most accomplished speech-readers.

I have been told by one who has acquired this art that when introduced to strangers their speech is more readily understood if they are not aware they are speaking to one who cannot hear. The moment they are told they commence to speak slowly and open their mouths to an unnatural extent, thus rendering their articulation partially unintelligible. The change brought about by the knowledge that the listener could not hear was sometimes sudden and great.

I have lately made an examination of the visibility of all the words in our language contained in a small pocket dictionary, and the result has assured me that there are glorious possibilities in the way of teaching speech-reading to the deaf, if teachers will give special attention to the subject.

One of the results of my investigation has been that the ambiguities of speech are confined to the little words, chiefly to monosyllables. The longer words are nearly all clearly intelligible. The reason is obvious, for the greater number of elements there are in a word the less likelihood is there that another word can be found that presents exactly the same outline to the eye.

We need never be afraid, therefore, of using long words to a deaf child, if they are within his comprehension. We are apt to have the idea that short words will be simpler, and we sometimes try to compose sentences consisting as much as possible of monosyllabic words, under the impression that such words are easy for the pupil to pronounce and read from the mouth. It is more common, therefore, to present such sentences to beginners than to more advanced pupils. Now, I do not mean to say that these sentences may not be easier for a child to pronounce, but the words used are the most ambiguous to the eye. Such a simple word as "man," for instance, is homophenous with no less than thirteen other words.

A few years ago I dictated a string of words to some pupils, with the object of testing whether they judged by context or were able to distinguish words clearly by the eye. The results are instructive. Among the words dictated occurred the following: "Hit—rate—ferry—aren't—hat—four—that—reason—high—knit—donned—co." I told the pupils not to mind whether they understood what I said or not, but simply to write down what they thought the words looked like, and what do you think they wrote? Upon examining their slates I found that nearly every child had written the following sentence: "It rained very hard, and for that reason

I did not go." I told the pupils to be very careful to observe whether they could distinguish any difference between the words I uttered and the words they wrote. I therefore went over the whole string of words again, articulating them one by one very distinctly. No difference whatever was detected.

The mother of one of my pupils was present, and was greatly astonished to see her daughter writing down words so different from those I had pronounced. She said that she could not have believed that her daughter could have been so stupid; but her surprise was increased when she found that the other children had written the same sentence. I told her that there was no difference in appearance between the words I had uttered and the words they had written. She desired to test the matter herself with her own child. She asked her daughter to repeat after *her* the words I had written, but the result was the same. The last part of the sentence she repeated at least a dozen times, without shaking her daughter's confidence in the belief that the words she had uttered were precisely the same as those spoken by her mother. To one who could hear, it was a startling revelation to observe the confidence of the child in the accuracy of her replies.

"Repeat after me," said the mother, as she pronounced the words singly and with deliberate distinctness: "high;" answer, "I; "knit," ans., "did;" "donned," ans., "not;" "co," ans., "go.' "Are you sure you have pronounced the words exactly as I have said them?" Ans. "Yes; perfectly certain." "Try again." "Knit," answer, "did;" "donned," answer "not." "Are you *sure* I said that?" Ans. "Yes; absolutely sure." "Try again," and here the mother mouthed the word "donned," ans., "not." The mother was convinced, and she left the room with the remark that she felt that she had been very cruel to her child through ignorance of the fact that words that were very different to her ear looked alike to her child, and could not possibly be distinguished, excepting by context.

I have seen a teacher attempting to impart instruction to a deaf child by word of mouth. She would speak word by word, and the pupil would repeat after her. Upon one occasion the pupil gave utterance to a very different word from that which had been spoken by the teacher. The latter repeated the word a number of times, opening her mouth to the widest extent, and the boy each time repeated the incorrect expression. The teacher grew annoyed at the supposed stupidity of the pupil, and the pupil grew sulky, and was discouraged in his attempt to read from the mouth; whereas, in reality, it was not the stupidity of the boy that was in the way of his progress, but the ignorance of the teacher, who did not know that the words that were so different to her ear were absolutely alike to his eye.

Some teachers, in their anxiety to teach speech-reading to their pupils, have the idea that they should refrain from every other mode of communication, so that their pupils may be forced to observe the movements of the mouth, and the mouth alone. For instance, it is easy to write an ambiguous word or to spell it by a manual alphabet, but some teachers refrain from doing so, under the impression that this practice leads the pupil to depend upon the hand instead of the mouth.

Again, deaf persons gather an idea of the emotion that actuates a speaker by the expression of his countenance. In fact facial expression is to the eye what the modulation of the voice is to the ear. It gives life to the inaudible utterances of the mouth; but there are some teachers who are so afraid that their pupils may come to depend upon the face instead of the mouth, that they think they should assume an impassive countenance from which nothing could be inferred.

Requisites to the Art of Speech-reading.

If we examine the visibility of speech and the causes of its intelligibility, we shall find that there are three qualifications that must be possessed by a deaf child in order that he may understand readily the utterances of his friends. Omit any one of these qualifications and good speech-reading is an impossibility:

I. The eye must be trained to recognize readily those movements of the vocal organs that are visible. Has this ever been done? Have not pupils been required to grapple with all the difficulties of speech-reading at once, and to observe not only the movements of the vocal organs, but to find out the meaning of what is said?

II. I have already explained that certain words have the same appearance to the eye, and it is necessary, if the pupil is to understand general conversation, that he shall know the words that look alike, so that a given series of movements of the vocal organs shall suggest to his mind not a single word, but a group of words, from which selection is to be made by context.

An illustration will explain what I mean. There are many words which have the same sound to the ear, but different significations. For instance, were I to ask you to spell the word "rāne," you could not tell whether I meant "rain," "rein," or "reign." These words sound alike, but they lead to no confusion, for they are readily distinguished by context. In the same way "homophenous words," or words that have the same appearance to the eye, are readily distinguished by context.

As a general rule when a teacher finds that her pupil does not understand a given word, she supposes the non-comprehension to be due to an untrained eye, and this leads to the patient repetition of the word with widely opened mouth, to make the action of the organs more visible. This, unintentionally, enables the pupil to acquire a knowledge of homophenous words; for, when he fails to understand in the first instance, he is requested to try again. He then guesses at the meaning. He thinks of all the words that past experience has taught him looked something like the word proposed, and after a series of guesses generally succeeds in his attempt to unravel the meaning.

In this way success comes at last, not in consequence of the pupil seeing more than he saw at first, but in consequence of knowledge gained by experience of failure. He learns what words present the same appearance to the eye. Let teachers find out the words that look alike, and teach them in groups to their pupils. In this way instruction will take the place of painful experience.

III. The third requisite to good speech-reading is familiarity with the English language. Familiarity with our language, either in its written or spoken form, is absolutely essential in order that a deaf person may make use of context in his attempt to decipher our speech. It is a mental problem that the deaf child has to solve and not solely a problem of vision. The eyes of the congenitally deaf, if there is any difference at all, are rather stronger and better than the eyes of those who become deaf from disease; and yet, as a class, the congenitally deaf acquire the art of speech-reading with much more difficulty than those who could speak before they became deaf. The reason is, that, as a class, the former have not a vernacular knowledge of our language even in its written form, while the latter have. Children who become deaf in infancy from disease are at as great a disadvantage in this respect as the congenitally deaf, and for the same reason.

I shall inquire more particularly into the cause of this lack of familiarity with the English language, and I shall show that it results from a wide-spread fallacy regarding the nature of language and the means by which our language should be taught. In the meantime I shall simply direct attention to the fact that those who are deaf from infancy do not, as a general rule, become familiar with the English language even in its written form.

It is obvious that if we talk to deaf children by word of mouth, and refrain from explaining, by writing or some other clearly visible means, the words that are ambiguous, those pupils who are already familiar with the language have very great advantages over the others. They have a fund of words from which to draw, they can guess at the ambiguous word and substitute other words within their knowledge so as finally to arrive at the correct meaning. But young children who have been deaf from infancy and who never, therefore, have known our language, are not qualified at once for this species of guess-work. They know no words excepting those we teach them, and have, therefore, no fund to draw upon in case of perplexity. If we commence the education of such children by speech-reading alone they are plunged into difficulties to which they have not the key.

To such children it becomes a matter of absolute necessity that our language should be presented to them in an unambiguous form. With such pupils, writing should be the main reliance, and speech-reading can only be satisfactorily acquired by the constant accompaniment of writing, or its equivalent—a manual alphabet. I have no hesitation in saying that the attempt to carry on the general education of young children who are deaf from infancy by means of articulation and speech-reading alone, without the habitual use of English in a more clearly visible form, would tend to retard their mental development. I do not mean to say that this is ever actually done, but I know there is a tendency among teachers of articulation to rely too much upon the general intelligibility of their speech. Let them realize that the intelligibility is almost entirely due to context, and they will rely more upon writing and less upon the mouth in their instructions to young congenitally deaf children.

After a probationary period, pupils who could speak before they became deaf become so expert in speech-reading that the regular instruction of the school-room can be carried on through its means without detriment to the pupil's progress. The exceptional cases of congenitally deaf persons who have become expert in this art assures us that, with all who are deaf from infancy, we can certainly achieve the same results if only we can give them a sufficient knowledge of our language, at least in its written form. In the early stages of the education of the congenitally deaf it appears to me that written English should be made the vernacular of the school-room, and that all words or sentences written should also be spoken by the teacher and read by the pupils from the mouth. When the English language has become vernacular there is no reason why instruction should not also be given by word of mouth alone (as in the case of those who could speak before they became deaf) without interfering with mental development.

Before leaving this subject I would say that it is of importance to remember that speaking and understanding speech by the eye are two very different things. We can all of us speak very readily, but I fancy it would puzzle most of us to be called upon to tell what a speaker says by watching his mouth. The congenitally deaf can certainly be taught to speak intelligibly even by persons unfamiliar with the mechanism of articulation. Such pupils should therefore be taught to articulate, and their vocal organs should be continually exercised in the school-room by causing them to speak as well as to write. The congenitally deaf can be taught to articulate even *before* they are familiar with English, but I do not think they can acquire the power of understanding ordinary conversational speech by watching the mouth, at least to any great extent, until *after* they have become familiar with our language.

Gesture Language.

I have already stated that the old fallacy, "without speech there can be no reason," prevented for hundreds of years any attempt at the education of the deaf and dumb, and now I come to the memorable experiment that forever exploded the fallacy. Towards the latter end of the last century the Abbe de l'Epee, during the course of his ministration in Paris, entered a room in which two girls were sewing. He addressed some remarks to them, but received no reply. These girls were deaf and dumb. At once the kind heart of the good Abbe was touched, and he determined to devote his life to the amelioration of the condition of the deaf and dumb.

He gathered together quite a number of deaf children, who made their home with him. He spent his time in their society and devoted to their comfort all that he possessed, reducing himself even to poverty for their sake. He soon observed that these children were communicating with one another, but not by speech. They were inventing a language of their own, unlike any of the spoken languages of the earth—a language of gestures. These children were reasoning by means of this language; they were thinking in gestures instead of in words, and the idea occurred to the Abbe de l'Epee that the old dogma that had for so many hundred years prevented the education of the deaf was a fallacy. Here was nature developing an instrument of reason with which speech had nothing to do. Why should he not study this gesture language and assist these children in their attempts to perfect a means of communication of this kind, and why should he not use this means of communication so as to lead their minds to higher and ever higher thoughts? He did so and succeeded in developing the "sign language" that is now so extensively employed in this country in the education of the deaf. The experiment at once attracted attention. Kings and Emperors visited the humble abode of the Abbe de l'Epee and were astonished by what they saw. He conversed with his pupils in the gesture language, and he taught them through its means the meaning of written French, so that they were enabled to communicate with hearing persons by writing.

The Fallacy that a Gesture Language is the only Form of Language that is Natural to the Congenitally Deaf.

The old fallacy was done away with, but a new one immediately took its place, which has been introduced into our country with the language of signs, and is now the main obstacle to the acquisition of English by the congenitally deaf. The fallacy to which I allude is that this gesture language is the only language that is natural to the congenitally deaf, and that therefore such children must acquire this language as their vernacular before learning the English language, and must be taught the meaning of the latter through its means. To my mind such a statement consists of a succession of fallacies, each one resting on the preceding. The proposition that the sign language is the only language that is natural to congenitally deaf children is like the proposition that the English language is the only language that is natural to hearing children. It is natural only in the same sense that English is natural to an American child. It is the language of the people by whom he is surrounded. A congenitally deaf child who for the first time enters an institution for the deaf and dumb finds the pupils and teachers employing a gesture language which he does not understand; but in time he comes to understand it, and learns by imitation to use it, just as an American child in Germany comes in time to understand and speak German.

Although congenitally deaf children, when they enter an institution, do not understand or use the sign language as there employed, they each know and use a gesture language of some kind, which they employ at home in communicating with their friends and relatives. Hence it is argued that if the "sign language" employed in our institutions is not the only one, a gesture language of some kind is necessarily the vernacular of the congenitally deaf child. The scope of the statement is thus widened, and the proposition we have now to consider may be thus expressed: Gesture language, in the wider sense, is the only form of language that is natural to those who are congenitally deaf.

It is a matter of great importance to the 34,000 deaf-mutes of this country, and to their friends and relatives, as well as to all persons who are interested in the amelioration of the condition of the deaf and dumb, that we examine this proposition with care and decide whether it is a fallacy or not. To my mind it is a fallacy based upon another concerning the nature of language itself, namely, that there is such a thing as a natural language. Such an idea has led to errors in the past, and will ever continue to do so. We have all read of the monarch of ancient times, who is recorded to have shut up a number of little children by themselves, and to have given orders to their attendants to hold no communication with them, so that he might observe what language they would naturally speak as they grew up. It is recorded that the first word uttered was a Greek word, from which it was argued that the Greek language was the natural language of mankind.

In the seventeenth century the ingenious Van Helmont was imbued with the idea that the Hebrew language was of divine origin, from which he argued that Hebrew was the natural language of mankind, and that the shapes of the Hebrew letters had some natural relation to the sounds they represented; that they pictured, in fact, the positions of the vocal organs in forming the sounds. The latter idea led him to employ the characters as a means of teaching articulation to a deaf-mute; but the former idea led him to teach his deaf-mute Hebrew, instead of his native tongue.

When we examine the languages of the world that are naturally acquired by hearing children, we fail to discover any natural connection between the sounds of the words and the things they represent; everything is arbitrary and conventional.

Origin and Mode of Growth of a Gesture Language.

Now, let us examine for a moment the nature of a gesture language and the manner in which it comes into existence. You are, we shall suppose, a farmer, and your little deaf boy comes running into the house in great excitement, anxious to tell you something he has observed. How does he do so?

We shall imagine a case. He commences by placing his hands above his head, bowing low, and marching about the room, after which he points out of the window.

You shake your head; you have not the remotest idea what he means.

His face assumes an anxious look, and down he goes upon his hands and knees, and scrambles over the floor, touching the carpet with his mouth from time to time, and then again points out of the window.

Still you do not comprehend.

A look of perplexity crosses his face. What can he do to make you understand? At last his face lights up, as a new thought comes into his mind, and he touches the bridge of his nose and again points out of the window.

But, alas! alas! you cannot understand.

The little fellow is perplexed and troubled. At last, in despair, he takes hold of your coat and pulls you out of the door, around the corner, and *you find your cow in the turnip patch.*

Now you begin to understand what it was he meant to say; he had tried to picture the cow, and to imitate its actions. The hands held above the head had indicated the horns; the scrambling on the floor on his hands and knees had imitated the action of a four-footed animal, and his mouth to the carpet meant the cow eating the turnips.

But how about the bridge of his nose?

You will probably observe that the cow to which he referred had some white spot or other mark upon the nose, and the gesture of the child had not indicated a cow in general, but your black cow "Bessie," with the white spot on her nose, in particular.

Having advanced thus far in the comprehension of his meaning, do you think that the child will take the trouble to go through this same pantomime the next time he wishes to tell you about your cow? No. He may commence such a pantomime, but before he gets half through you understand what he means, and he never completes it. A process of abbreviation commences, until finally a touch on the bridge of his nose alone becomes the name of your black cow "Bessie," and the simple holding of his hands above his head conveys to your mind the idea of a cow in general.

By a natural process of abbreviation the child arrives at a simple gesture or sign for every object or thing in which he is interested.

But there are many thoughts he desires to express which are abstract in their nature. How, for instance, can he indicate by any sign the color of an object? Suppose, by way of illustration, that he desired to communicate to you the idea that he had seen in the road a cow that was perfectly white?

I shall try to depict the conversation between yourself and your deaf boy as it might actually have occurred.

THE BOY. The boy points to the road, touches his teeth, and holds his hands above his head.

You gather from this a vague idea of some connection between that road, the boy's teeth, and a cow.

Here is a problem: What did he mean? It is pretty clear that he had seen a cow in the road, but what connection had his teeth with that? Perhaps the cow's teeth were peculiar. You think you had better get him to explain, so—

THE FATHER. You touch your teeth with an interrogative and puzzled look.

THE BOY. The boy responds by showing you his shirt sleeve and pointing to the road.

Can he mean that there was any connection between his shirt sleeve and the cow. To clear this point—

THE FATHER. You touch his shirt sleeve and raise your hands above your head with a look of interrogation.

THE BOY. The boy nods vigorously, raises his hands above his head, and makes his sign for "snow," followed by other signs for objects that are white.

After he has presented a sufficient number of such signs, you perceive that the one thing common to them all was their color—they were white. And thus you gain the idea that the cow was white.

Do you suppose he goes through this process every time he desires to communicate the idea of white? No; he remembers the object which had conveyed to your mind the idea that that cow was white, and the sign for this object is ever after used as an adjective, qualifying the object the whiteness of which he desires to indicate. Of course you cannot predicate what this particular sign may be. I have seen children who have conveyed the idea by touching their teeth; others who expressed it by an undulatory downward movement of the hand, expressive of the way in which a snow-flake falls to the ground.

It will thus be understood that a deaf child first commences to express his ideas by pantomime, and that by a process of abbreviation pantomimic gestures come to be used in a conventional manner. Pantomime is no more entitled to the name of language than a picture is, although many ideas can be conveyed through its means. In proportion as it becomes more conventional and arbitrary it becomes more and more worthy of the name of language.

The Sign-Language of Our Institutions.

Now, when the deaf children who lived with the Abbe de l'Epee were first brought together, each of them used a gesture-language he had invented for himself as a means of communicating with his friends at home. Thus there were as many gesture-languages as there were children. The only element common to these languages was probably the pantomime from which they had all sprung. But now what happened? Association and the necessity of intercommunication led to the adoption of common signs. Each child presented his gestures to his fellows, and by a process of selection those signs that appeared to the majority to be most fitting survived, and were adopted by the whole; and the synonymous signs, which were not so well fitted, were either forgotten by disuse or used in a new meaning to express other ideas.

I do not wonder at the interest displayed in this growth by the Abbe de L'Epee and his contemporaries. To my mind it was the most interesting and instructive spectacle that has ever been presented to the mind of man—*the gradual evolution of an organized language from simple pantomime.*

When, in 1817, the first school for the deaf and dumb was opened in America, the sign-language as used in the school of the Abbe de l'Epee (then under the charge of his successor, the Abbe Sicard) was imported from France, and became the medium of instruction. The teachers trained in this school naturally became the principals of other institutions established upon its model, and thus the sign-language has been diffused over the length and breadth of our land.

I heartily agree with all that experienced teachers of the deaf have urged concerning the beauty and great interest of this gesture language. It is indeed interesting to observe how pantomimic gestures have been abbreviated to simple signs expressive of concrete ideas; how these have been compounded or have changed their meaning to indicate abstract thoughts; and how the sequence of the sign-words has to a certain extent become obligatory, thus forming a sort of gesture syntax or grammar.

The original stock or stocks from which our languages are derived must have disappeared from earth ages before historic times; but in the gesture speech of the deaf we have a language whose history can be traced *ab origine*, and it has appeared to me that this fact should give it a unique and independent value. In the year 1878, in a paper read before the Anthropological Society of London, I advocated the study of the gesture language by men of science; for it seemed to me that the study of the mode in which the sign language has arisen from pantomime might throw a flood of light upon the origin and mode of growth of all languages.

You may ask why it is that, with my high appreciation of this language *as a language*, I should advocate its entire abolition in our institutions for the deaf.

I admit all that has been urged by experienced teachers concerning the ease with which a deaf child acquires this language, and its perfect adaptability for the purpose of developing his mind; but after all it is not the language of the millions of people among whom his lot in life is cast. It is to them a foreign tongue, and the more he becomes habituated to its use the more he becomes a stranger in his own country.

This is not denied by teachers of the deaf and dumb, but the argument is made, as I have stated above, that it is the only language that is natural to congenitally deaf children, or that at all events, some form of gesture language must necessarily be their vernacular, and be employed to teach our English tongue.

The Fallacy that a Gesture Language is the only form of Language in which a Congenitally Deaf Child can Think.

Now what do we mean by a language being "natural" or not? I cannot believe that in this 19th century any one really entertains the fallacy that there is a natural language *per se*. So I presume that that language is considered natural to a person in which he thinks. Under this meaning the proposition assumes this shape: The sign language taught in our institutions, or a gesture language of some kind, is the only form of language in which a congenitally deaf child can think; that is, it is the only language of which the elements can be associated directly with the ideas they express.

In this form the fallacy is easily exploded, for in the course of the last one hundred years so many experiments have been made in the education of the deaf that we now know with absolute certainty that deaf children can be taught to associate written words directly with the ideas they represent; and when they are taught to spell these words by a manual alphabet, the movements of the fingers become so natural a method of giving vent to their thoughts that even in sleep their fingers move when they dream.

Not only has written English been made the vernacular of congenitally deaf children, but the same result has been achieved with written French, German, Spanish, Dutch, and other languages.

Congenitally deaf children who have been taught articulation move their mouths in their sleep and give utterance to words when they dream.

Laura Bridgman, the blind deaf-mute, was taught by the late Dr. Howe to gather ideas through the sense of touch. English words printed in raised letters were presented to her sense of touch in connection with the objects which they represented, and she associated the impressions produced upon the ends of her fingers with the objects themselves. The English language in a *tangible* form became her vernacular.

All these facts assure us that any form of language may become natural to a deaf child by usage, so long as it is presented to the senses he possesses. There is only one way that language is naturally acquired, and that is by usage and imitation. Any form of language that can be clearly appreciated by the senses the deaf child possesses, will become his vernacular if it is used by those about him.

Why the Deaf employ a Gesture Language.

A gesture language is employed by a deaf child at home, not because it is the only language that is natural to one in his condition, but because his friends neglect to use in his presence any other form of language that can be appreciated by his senses. Speech is addressed to his ear; but his ear is dead, and the motions of the mouth cannot be fully interpreted without previous familiarity with the language. On account, therefore, of the neglect of parents and friends to present to his eye any clearly visible form of language, the deaf child is forced to invent such a means of communication, which his friends then adopt by imitation. I venture to express the opinion that no gesture language would be developed at home by a deaf child if his. parents and friends habitually employed, in his presence, the English language in a clearly visible form. He would come to understand it by usage, and use it by imitation.

An old writer, George Dalgarno, in 1680, expressed the opinion, in which I fully concur, that "there might be successful addresses made to a dumb child even in its cradle, *risu cognoscere matrem*, if the mother or nurse had but as nimble a hand as usually they have a tongue."

When deaf children enter an institution they find the other pupils and the teachers using a form of gesture language which they do not understand. For the first time in their lives they find a language used by those about them that is addressed to the senses they possess. After a longer or shorter time they discard the language that they had themselves devised, and acquire, *by imitation*, the sign language of the institution.

Harmful Results of the Sign Language.

After a few months residence in the institution, the children return to their friends in the holidays using easily and fluently a language that is foreign to them, while of the English language they know no more than the average school boy does of French or German after the same period of instruction. The only language they can employ in talking to their friends is the crude gesture language of their own invention, which they had long before discarded at school; and they perpetually contrast the difficulty and slowness of comprehension of their friends with the ease with which their school fellows and teachers could understand what they mean. They have learned by experience how sweet a thing it is to communicate freely with other minds, and they are continually hampered and annoyed by the difficulty they meet with in conversing with their own parents and friends.

Can it be wondered at, therefore, that such a child soon tires of home? He longs for the school play-ground, and the deaf companions with whom he can converse so easily. Little by little the ties of blood and relationship are weakened, and *the institution becomes his home.*

Nor are these all the harmful effects that are directly traceable to the habitual use in school, as a means of communication, of a language foreign to the mass of the people. Disastrous results are traceable inwards in the operation of his mind, and outwards in his relation to the external world in adult life. He has learned to *think* in the gesture-language, and his most perfected English expressions are only translations of his sign speech.

As a general rule, when his education is completed, his knowledge of the English language is like the knowledge of French or German possessed by the average hearing child on leaving school. He cannot read an ordinary book intelligently without frequent recourse to a dictionary. He can understand a good deal of what he sees in the newspapers, especially if it concerns what interests him personally, and he can generally manage to make people understand what he wishes by writing, but he writes in broken English, as a foreigner would speak.

Let us consider for a moment the condition of a person whose vernacular is different from that of the people by whom he is surrounded. Place one of our American school boys just graduated from school in the heart of Germany. He finds that his knowledge of German is not sufficient to enable him to communicate freely with the people. He thinks in English, and has to go through a mental process of translation before he can understand what is said, or can himself say what he means. Constant communication with the people involves constant effort and a mental strain. Under such circumstances what a pleasure it is for him to meet with a person who can speak the English tongue. What a relief to be able to converse freely once more in his own vernacular. Words arise so spontaneously in the mind that the thought seems to evoke the proper expression.

But mark the result: the more he associates with English-speaking people the less desire does he have to converse in German. The practice of the English language prevents progress in the aquisition of German. I have known of English people who have lived for twenty years in Germany without acquiring the language.

If our American school boy desires to become familiar with the German language, he must resolutely avoid the society of English-speaking people. He then finds that the mental effort involved in conversation becomes less and less, until, finally, he learns to think in German, and his difficulties cease.

Now consider the case of a deaf boy just graduated from an institution where the sign language has been employed as a means of communication. His vernacular is different from that of the people by whom he is surrounded. He thinks in the gesture language and has to go through a mental process of translation before he can understand what is said or written to him in English, and before he can himself speak or write in English what he desires to say. He finds himself in America, in the same condition as that of the American boy in Germany. If he avoids association with those who use the sign language, and courts the society of hearing persons, the mental effort involved in conversation becomes less and less, and finally he learns to think in English and his difficulties cease.

But such a course involves great determination and perseverance on the part of the deaf boy, and few, indeed, are those who succeed.

Not only do the other deaf-mutes in his locality have the same vernacular as his own, but they were his school fellows, and they have a common recollection of pleasant years of childhood spent in each other's society. Can it be wondered at, therefore, that the vast majority of the deaf graduates of our institutions keep up acquaintance with one another in adult life? The more they communicate with one another the less desire they have to associate with hearing persons, and the practice of the gesture language forms an obstacle to further progress in the acquisition of the English language.

These two causes (*a*) previous exclusive acquaintance with one another in the same school, and (*b*) a common knowledge of a form of language specially adapted for the communication of the deaf with the deaf, operate to attract together into the large cities large numbers of deaf persons, who form a sort of deaf community or society, having very little intercourse with the outside world.

They work at trades or businesses in these towns, and their leisure hours are spent almost exclusively in each other's society. Under such circumstances can we be surprised that the majority of these deaf persons marry deaf persons, and that we have as a result a small but necessarily increasing number of cases of hereditary deafness due to this cause. Such unions do not generally result in the production of deaf offspring, because the deafness of the parents in a large proportion of cases is of accidental origin, and accidental deafness is no more likely to be inherited than the accidental loss of a limb. Still I would submit that the constant selection of the deaf by the deaf in marriage is fraught with danger to the community.

Why the English Language should be Substituted for the Sign Language as a Vernacular.

If we examine the position in adult life of deaf children who have been taught to speak, or who have acquired the English language as a vernacular, whether in its written or spoken forms, we find an entirely different set of tendencies coming into play, especially if these persons have not been forced in childhood to make the acquaintance of large numbers of other deaf children, by social imprisonment for years together in the same school or institution apart from the hearing world.

Their vernacular use of the English language renders it easy for them to communicate with hearing persons by writing, or by word of mouth if they have been taught to articulate; and hearing persons can easily communicate with them by writing, or by word of mouth if they have been taught the use of the eye as a substitute for the ear. The restraints placed upon their intercourse with the world by their lack of hearing leads them to seek the society of books, and thus they tend to rise mentally to an ever higher and higher plane. A cultivated mind delights in the society of educated people, and their knowledge of passing events derived from newspapers forms an additional bond of union between them and the hearing world.

If they have formed in childhood few deaf acquaintances, they meet in after life hundreds of hearing persons for every deaf acquaintance, and if they marry, the chances are immensely in favor of their marrying hearing persons.

There is nothing in the deaf-mute societies in the large cities to attract them, and much to repel them; for the more highly educated deaf-mutes in these societies speak what is to them a foreign language; while the greater number of the deaf-mutes to be found there are so ignorant that self-respect forbids them from mingling with them.

Thus the extent of their knowledge of the English language is the main determining cause of the congregation or separation of the deaf in adult life. A good vernacular knowledge of the English language operates to effect their absorption into society at large, and to weaken the bonds that tend to bring them together; whereas, a poor knowledge of the language of the country they live in causes them to be repelled by society and attracted by one another; and these attractive and repulsive tendencies are increased and intensified if they have been taught at school a language foreign to society and specially adapted for intercommunication among themselves. I say, then, let us banish the sign language from our schools. Let the teachers be careful in their intercourse with their pupils to use English and English alone. They can write, they can speak by word of mouth, they can spell the English words by a manual alphabet, and by any or all of these methods they can teach English to their pupils as a native tongue.

Conclusion.

In conclusion allow me to say:

1. That those whom we term "deaf-mutes" have no other natural defect than that of hearing. They are simply persons who are deaf from childhood and many of them are only "hard-of-hearing."

2. Deaf children are dumb, not on account of lack of hearing, but of lack of instruction. No one teaches them to speak.

3. A gesture language is developed by a deaf child at home, not because it is the only form of language that is natural to one in his condition, but because his parents and friends neglect to use the English language in his presence in a clearly visible form.

4. (*a*) The sign language of our institutions is an artificial and conventional language derived from pantomime.

(*b*) So far from being natural either to deaf or hearing persons, it is not understood by deaf children on their entrance to an institution. Nor do hearing persons become sufficiently familiar with the language to be thoroughly qualified as teachers until after one or more years' residence in an institution for the deaf and dumb.

(*c*) The practice of the sign language hinders the aquisition of the English language.

(*d*) It makes deaf-mutes associate together in adult life, and avoid the society of hearing people.

(*e*) It thus causes the intermarriage of deaf-mutes and the propagation of their physical defect.

5. Written words can be associated directly with the ideas they express, without the intervention of signs, and written English can be taught to deaf children by usage so as to become their vernacular.

6. A language can only be made vernacular by constant use as a means of communication, without translation.

7. Deaf children who are familiar with the English language in either its written or spoken forms can be taught to understand the utterances of their friends by watching the mouth.

8. The requisites to the art of speech-reading are:

(*a*) An eye trained to distinguish quickly those movements of the vocal organs that are visible (independently of the meaning of what is uttered.)

(*b*) A knowledge of *homophenes;** that is, a knowledge of those words that present the same appearance to the eye; and

(*c*) Sufficient familiarity with the English language to enable the speech-reader to judge by context which word of a homophenous group is the word intended by the speaker.

If we look back upon the history of the education of the deaf, we see progress hindered at every stage by fallacies. Let us strive, by discussion and thought, to remove these fallacies from our minds so that we may see the deaf child in the condition that nature has given him to us. If we do this, I think we shall recognize the fact that the afflictions of his life are *mainly due to ourselves*, and we can remove them.

Nature has been kind to the deaf child, man cruel. Nature has inflicted upon the deaf child but one defect—imperfect hearing; man's neglect has made him dumb and forced him to invent a language which has separated him from the hearing world.

Let us, then, remove the afflictions that we ourselves have caused.

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* This word was suggested to me some years ago by Mr. Homer, lately Principal of the Providence (R. I.) School for Deaf-Mutes, and has now been permanently adopted.

1. Let us teach deaf children to think in English, by using English in their presence in a clearly visible form.

2. Let us teach them to speak by giving them instruction in the use of their vocal organs.

3. Let us teach them the use of the eye as a substitute for the ear in understanding the utterances of their friends.

4. Let us give them instruction in the ordinary branches of education by means of the English language.

5. And last, but not least, let us banish the sign language from our schools.

If it were our object to fit deaf children to live together in adult life and hold communication with the outside world as we hold communication with other nationalities than our own, then no better plan could be devised than to assist the development of a special language suitable for intercommunication among the deaf.

But if, on the other hand, it is our object to destroy the barriers that separate them from the outside world and take away the isolation of their lives, then I hold that our energies should be devoted to the acquisition of the English language as a vernacular in its spoken and written forms. With such an object in view we should bring the deaf together as little as possible and only for the purpose of instruction. After school hours we should separate the deaf children from one another to prevent the development of a special language and scatter them among hearing children and their friends in the outside world.

The subject being presented to the Society for discussion, Mr. E. M. Gallaudet spoke, in substance, as follows:

I have listened with great interest to the remarks of Mr. Bell this evening, and am ready to agree in many particulars with the views he has so well presented.

I am, however, compelled to differ with him at several points; and as these involve matters of vital importance in the treatment of the deaf, I will beg the indulgence of the Society for a short time, while I attempt to show to what extent some of Mr. Bell's views are erroneous.

In proving the generally received opinion that the vocal organs of persons deaf from infancy are defective, to be a fallacy, Mr. Bell declared that difficulties encountered by such persons in acquiring speech are wholly external to themselves, and that all persons so situated can, with proper instruction, be taught to speak and to understand the motions of the lips of others.

That this is a grave error has been proved by the experience of more than a century of oral teaching in Germany.

The late Moritz Hill, of Wessenfels, Prussia, a man of the widest experience and highest standing among the oral teachers of Europe, expressed to me the opinion a few years since that out of one hundred deaf-mutes, including the semi-mute and semi-deaf, only "eleven could converse readily with strangers on ordinary subjects" on leaving school. Of course a much larger number would be able to converse with their teachers, family, and intimate friends on common-place subjects; but it would be found that very many could never attain to any ready command of speech.

The explanation of this lies in the fact that a child, deaf from infancy, in order to succeed with speech and lip-reading must possess a certain quickness of vision, a power of perception, and a control over the muscles of the vocal organs, by no means common to all such children.

Mr. Bell's view has been held by many instructors with more or less tenacity, and *this* fact is explained by a readiness on their part to argue from the particular to the general. Having attained marked success with certain individuals, they draw, in their enthusiasm, the mistaken conclusion that success is possible in the case of every other deaf child, overlooking the fact that many things, besides the mere deafness of the child, may affect the result. Experience has demonstrated that in attempting to teach the deaf to speak, failure in many cases must be anticipated.

Mr. Bell is mistaken in supposing ignorance as to the mechanism of the vocal organs to be a prominent cause of failure to impart speech to the deaf. It is no doubt true that among persons unfamiliar with the training of the deaf, few have made the mechanism of speech a study; but in Germany, Italy, and France, not to speak of our own country, many are to be found who may be said to have mastered this subject. The results of their labors have been made available to instructors of the deaf, and all the best oral schools are profiting thereby.

Mr. Bell is also mistaken when he says that "in a majority of our schools and institutions articulation and speech-reading are taught to only a favored few, and in these schools no use of articulation is made as a means of communication," and that "few, if any, attempts are made to teach articulation to those who have not naturally spoken." In most of the larger institutions for the deaf in this country, every pupil is afforded an opportunity to acquire speech, and instruction in this is discontinued only when success seems plainly unattainable.

It is a great error to suppose it to be true of a deaf person educated on what Mr. Bell calls the sign-method, that, "as a general rule, when his education is completed, his knowledge of the English language is like the knowledge of French or German possessed by the average hearing child on leaving school," or to say that "he cannot read an ordinary book intelligently without frequent recourse to a dictionary." On the contrary, a majority of persons thus educated have a good knowledge of their vernacular, are able to use it readily as a means of communication with hearing persons, and *are* able to read intelligently without frequent recourse to the dictionary.

When Mr. Bell has become familiar with the peculiarities of the deaf by personal contact with a large number of this class of persons, I am confident he will not repeat his assertion that "nature has inflicted upon the deaf child but one defect—imperfect hearing." For he will then have discovered, what has long been known to teachers of experience, that deaf children, in addition to their principal disability, are often found to be lacking in mental capacity, or in the imitative faculty, in the power of visual or tactile perception, and in other respects; all of which deficiencies, though they do not amount even to feeble-mindedness, much less to idiocy, do operate against the attainment of success in speech, as well as in other things which go to complete the education of such children.

Passing over several points of relatively small importance, in regard to which I believe Mr. Bell's views to be subject to criticism, I come to his characterization as a fallacy of the opinion held by many "that the language of gestures is the only language natural to the child born deaf or who has become deaf in infancy."

I think that in order to sustain his view that this is a fallacy Professor Bell gives a strained and very unusual meaning to the words "natural language." If, as he explains, a natural language is any one that a child may happen to be first taught by those with whom he is associated, then I should have no controversy with him. But I understand a natural language to be one that is mainly spontaneous, and not at all one that is borne in upon a child from without.

Moritz Hill, to whom I have already alluded, speaks of the language of signs as "one of the two universally intelligible innate forms of expression granted by God to mankind," the other being speech. Now it is hardly necessary to urge that speech is the form of expression natural to hearing persons, and I think a little reflection will satisfy most persons that with the deaf the language of signs is the only truly natural mode of expressing their thoughts.

Mr. Bell urges that the use of signs in the education of the deaf is a hinderance rather than a help, and that it would be better to banish them altogether. To this view I must give my earnest dissent.

I might, of course, cite the opinions of very many successful instructors of the deaf, who have followed only the sign method, to sustain my position, but I prefer to call in again the testimony of Moritz Hill, a man whose whole life was devoted to the instruction of the deaf by the oral method. In an exhaustive work on the education of the deaf,* Hill says, speaking of those who pretend that in the "German method" every species of pantomimic language is proscribed:

"Such an idea must be attributed to malevolence or to unpardonable levity. This pretence is contrary to nature and repugnant to the rules of educational science.

"If this system were put into execution the moral life, the intellectual development of the deaf and dumb, would be inhumanly hampered. It would be acting contrary to nature to forbid the deaf-mute a means of expression employed by even hearing and speaking persons. * * * It is nonsense to dream of depriving him of this means until he is in a position to express himself orally. * * * Even in teaching itself we cannot lay aside the language of gestures (with the exception of that which consists in artificial signs and in the manual alphabet—two elements proscribed by the German school), the language which the deaf-mute brings with him to school, and which ought to serve as a basis for his education. To banish the language of natural signs from the school-room and limit ourselves to articulation is like employing a gold key which does not fit the lock of the door we would open and refusing to use the iron one made for it. * * * At the best, it would be *drilling* the deaf-mute, but not *moulding* him intellectually or morally."

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* Der gegenwärtige Zustand des Taubstummen Bildungswesens in Deutschland; von Hill, Inspector der Taubstummen Anstalt zu Wetssenfels; Ritter des St. Olafs, &c. Weimar, H. Böhlau, 1866.

Hill then follows with thirteen carefully formulated reasons why the use of signs is important and even indispensable in the education of the deaf.

Mr. Bell is in error when he supposes that in the so-called sign-schools verbal language is only imparted through the intervention of the sign-language. In many well-ordered schools of this class, language is taught without the use of signs, and in such schools the language of signs is kept in its proper position of subordination. It goes without saying that in schools for the deaf there may be an injudicious and excessive use of signs. This is always to be guarded against, and when it is, I am convinced that no harm, but great good, results from the use of signs in teaching the deaf.

Furthermore, it is well known that the attempt to banish signs from a school for the deaf rarely succeeds. Miss Sarah Porter, for three years an instructor in the Clarke Institution at Northampton, Mass., an oral school in which most excellent results have been attained, shows candor as well as judgment when she says, in a recent article in the *American Annals of the Deaf and Dumb*, "Every oral teacher knows that fighting signs is like fighting original sin. Put deaf children together and they will make signs secretly if not openly in their intercourse with each other."

It is not true as a matter of fact that the use of signs necessarily prevents the deaf from acquiring an idiomatic use of verbal language and from thinking in such language. Large numbers of them who have never been taught orally have come into such a use of verbal language, and while it is granted that many educated under the sign system do not use verbal language freely and correctly, the same is found to be true of very many who have been educated entirely in oral schools.

In one important particular the language of signs performs a most valuable service for the deaf, and one of which nothing has yet been found to take the place. Through signs large numbers of deaf persons can be addressed, their minds and hearts being moved as those of hearing persons are by public speaking in its various forms.

Having seen the good effects on the deaf of the discreet use of the sign-language through a period of many years, I am confident that its banishment from all schools for the deaf would work great injury to this class of persons intellectually, socially, and morally.

The Hon. GARDINER G. HUBBARD being present, was invited by the chair to participate in the discussion. He said he had been connected with the Clark Institution for many years. The deaf pupils in that school are taught entirely by articulation.

From recent inquiries which had been made to ascertain how far the graduates had profited by instruction in articulation, it appeared that in almost in every instance they could carry on conversation with others sufficiently to engage in many kinds of business from which they would have been excluded if they had only used signs.

It was true, as Mr. Gallaudet said, the congenitally deaf were frequenty able to articulate more distinctly than those who lost their hearing at an early age, but this arises from the fact that the disease that caused the deafness affected the organs of articulation to a greater or less degree; but the congenitally deaf do not make as rapid progress in their studies as those who had once spoken, for these have a knowledge of language which the former could obtain only by long protracted study.

Mr. Hubbard believed that the pupils at the Clark Institution made at least as rapid progress in all their studies as those taught by signs; while, at the same time, they acquired the power of reading from the lips and speaking, in which those taught by signs were deficient.

When the first application was made to the Legislature of Massachusetts for the incorporation of the Clark Institution, Mr. Dudley, of Northampton, chairman of the committee to whom the petition was referred, had a congenitally deaf child under instruction at Hartford. The petitioners were opposed by the professors from the asylum, as they believed an articulating school would retard the education of the deaf, as it was impractical to teach the deaf by articulation, that system having been tried and proved a failure, and the new method was stigmatized as one of the visionary theories of Dr. Howe, (the principal of the Perkins Institute for the Blind, and the teacher of Laura Bridgeman, the blind deaf mute,) who was associated with the petitioners in the hearing.

The application was rejected through the influence of these professors and of Mr. Dudley, who 'knew, from experience with his own child, that it was impossible to teach the congenitally deaf to talk.'

Two years after, our application was renewed and with better success.

Mr. Hubbard in the meantime, with the aid of Miss Rogers, had opened a small school where the deaf were taught to speak. This school was visited and examined by the committee, and the progress made was so great that Mr. Dudley became a warm convert, convinced that the impossible was possible, and the application was granted, although again opposed by the gentlemen from Hartford. The school was opened at Northampton, and has been in operation for nearly fifteen years, and teaching by articulation has ceased to be a visionary theory.

Many of the warmest friends of the Institution now are, like Mr. Gallaudet, connected with institutions where signs are used. In almost every institution for the deaf classes are now taught to articulate, though articulation is not used as the instrument for instruction.

Mr. Gallaudet had taken exception to the remark of Mr. Bell, that idiots were born dumb, and said that in every school for idiots there were many feeble-minded children who could talk readily; but Mr. Bell used the word idiot not as simply a feeble-minded person, but according to its ordinary meaning, "a human being destitute of reason or the ordinary intellectual powers of man."

It has always been the policy at Northampton to prevent, as far as possible, marriages of deaf with deaf, for the records show that the children of such intermarriages are often deaf; and even where a congenitally deaf person marries a hearing person, the children sometimes are deaf.

The tendency of the intermarriage of the deaf would be to raise a deaf race in our midst.

About one in 1,500 of the population are deaf; but if these intermarriages should take place and a deaf race be created, the proportion would rapidly increase. The object of all friends of the deaf should be to prevent the deaf from congregating, and to induce them to associate with hearing people. In bringing the deaf together in institutions, where they are taught by signs, the tendency is to make the deaf deafer and the dumb more dumb.

It was originally intended to have only a family or small school at Northampton, but it was soon found that signs could not be excluded from the play-ground, as the young children could not communicate in any other way. The plan was changed, the number of pupils was largely increased, and a preparatory department established, in which signs were tolerated on the play-ground. On the removal of the pupils to the higher departments, the use of signs is forbidden, and they are rarely used on the play-ground or between the pupils, either in or out of school hours.

In the later years of instruction they acquire great facility in articulation and reading from the lips, though there is almost always some difficulty for a stranger to understand them.

Mr. Gallaudet had referred to the International Convention of deaf-mute teachers and their friends, at Milan, three years ago. Mr. Hubbard was present at the convention held this year at Brussels, and was there informed that a delegate had been sent from France to attend the convention at Milan and investigate the method of instruction in Italy, where articulation was used, for the purpose of deciding whether the instruction in the French schools should continue to be by signs, or instruction by articulation be substituted for signs.

The preference of the delegate had been for signs, but on witnessing the results obtained in the Italian schools and hearing the discussion, he was led to advise that the instruction in the French schools hereafter be by articulation, instead of signs, and such a change has, Mr. Hubbard understands, been made in most of the schools of France.

Mr. Hubbard learned from the reports at Brussels that almost all the European schools were taught by articulation, and that this means of instruction was being rapidly substituted for the sign language in England as well as in France.

Mr. Bell, in reply to the remarks of Mr. Gallaudet, said:

There are signs *and* signs. There is the same distinction between pantomime and the sign-language that there is between a picture and the Egyptian hieroglyphics.

Pictures are naturally understood by all the world, but it would be illogical to argue from this that a picture-language, like that developed by the ancient Egyptians, must also be universally intelligible. Pantomime is understood by all the world, but who among us can understand the sign-language of the deaf and dumb without much instruction and practice?

No one can deny that pantomime and dramatic action can be used, and with perfect propriety, to illustrate English expressions so as actually to facilitate the acquisition of our language by the deaf; but the abbreviated and conventionalized pantomime, known as the "sign-language," is used *in place* of the English language, and becomes itself the vernacular of the deaf child.

Judging from the quotations given by Dr. Gallaudet, Moritz Hill himself makes a clear distinction between pantomime and the sign-language, retaining the former and proscribing the latter. "Every species of pantomimic language is not proscribed," he says. "Natural signs," or "signs employed by hearing and speaking persons," are retained, while "artificial signs" are proscribed.

All the arguments that have been advanced regarding pantomime and a pantomime language are equally applicable to pictures and a picture-language. For instance, we may say that a picture-language is more natural than any of the spoken languages of the world, because pictures are naturally understood by all mankind. We may even arrive, by a further process of generalization, at the idea that picture-language, in the wider sense, really constitutes the only form of language that is natural at all, for all the other languages of the world appear to be entirely arbitrary and conventional. If we pursue the parallel we shall arrive at the conclusion that a picture-language of some kind must necessarily become the vernacular of our pupils, through which the other more conventional languages may be explained and taught.

It is immaterial whether such statements are fallacious or not, so long as we do not apply them to educational purposes. But let us see how they work in practice. The exhibition of a picture undoubtedly adds interest to the fairy tale or story that we tell a child. It illustrates the language we use, and it may be of invaluable assistance to him in realizing our meaning. But is that any reason why we should teach him Egyptian hieroglyphics? Granting the premises: Is the conclusion sound that we should therefore teach him English *by means* of hieroglyphics?

If such conclusions are illogical, then the fundamental ideas upon which our whole system of education by signs is based are also fallacious and unsound.

One word in conclusion regarding speech.

The main cause of the fallacies that fog our conception of the condition of the deaf child is *his lack of speech*. A deaf person who speaks is regarded by the public more as a foreigner than as a deaf mute. Speech, however imperfect, breaks through the barriers of prejudice that separate him from the world, and he is recognized *as one of ourselves*.

Mr. Gallaudet under-estimates the value of speech to a deaf child. He seems to think that speech is of little or no use, unless it is as perfect as our own. The fact is that the value of speech to a deaf child must be measured by its *intelligibility* rather than by its perfection.

It is astonishing how imperfect speech may be and yet be intelligible. We may substitute a mere indefinite murmur of the voice for all our vowel sounds, without loss of intelligibility. (Here Mr. Bell spoke a few sentences in this way, and was perfectly understood.) Here at once we get rid of the most difficult elements we are called upon to teach. If now we examine the relative frequency of the consonantal elements, we shall find that 75 per cent. of the consonants we use are formed by the point of the tongue, and that the majority of the remainder are formed by the lips. The consonants that are difficult to teach are chiefly formed by the top or back part of the tongue; but, on account of their comparative rarity of occurrence, they may be very imperfectly articulated without loss of intelligibility. Hence I see no reason why, in spite of the general ignorance of teachers respecting the mechanism of speech, we may not hope to teach all deaf children an intelligible pronunciation.

Let teachers appreciate the value of intelligible speech to a deaf child, and they will make the attempt to give it to him. At the present time, lack of appreciation operates to prevent the attempt from being made upon a large scale. Skilled teachers of articulation will become more numerous as the demand for their service increases, and their ingenuity, intelligently applied, will increase the perfection of the artificial speech obtained.

In the meantime, do not let us discard speech from the difficulty of obtaining it in perfection. Do not let us be misled by the idea that intelligible but defective speech is of no use, and must necessarily be painful and disagreeable to all who hear it. Those who have seen the tears of joy shed by a mother over the first utterances of her deaf child will tell you a different tale. None but a parent can fully appreciate how sweet and pleasant may be the imperfect articulation of a deaf child.

240TH MEETING. NOVEMBER 10, 1883.

The President in the chair.

Forty-eight members present.

Announcement was made of the election to membership of ETHELBERT CARROLL MORGAN.

It was announced from the General Committee that invitation had been extended to the members of the Anthropological and Biological Societies to attend the meeting of December 8th, for the purpose of listening to the annual address of the President.

Mr. EDWIN SMITH exhibited a

SEISMOGRAPHIC RECORD OBTAINED IN JAPAN,

describing the apparatus by which it was made, and giving a brief account of the seismographic investigations of Professor J. A. Ewing.

Remarks were made by Mr. ANTISELL.

Mr. C. E. DUTTON made a communication, entitled

THE VOLCANIC PROBLEM STATED.

[Abstract.]

It is sufficiently obvious that the volcano is a heat problem, or a thermo-dynamic problem. All volcanic activity is attended with manifestations of great energy. This energy is due to the elastic force of considerable quantities of water occluded in red-hot or yellow-hot lavas. The problem is to find a satisfactory explanation of the origin of the heat, the origin of the occluded water, and their modes of reaction.

In attempting this solution, various explanations have been conjectured. The first to be noticed, and the one which, in various forms, has met with the most favor from geologists and physicists, is that the source of heat is primordial—*i. e.*, it is the remains of a large amount of heat contained by the entire earth-mass in its supposed primordial condition, according to the nebular hypothesis; that water has penetrated from above, either from the ocean or from lakes; and that the contact of cold water with the hot magmas within the earth is a summary explanation of the phenomena. This view is supported by the following considerations: 1st, the contact of water with intensely hot bodies and the resulting generation of great explosive force is matter of the commonest experience; 2d, the outer rocks and strata are known to be full of fissures, and the ocean bottom and lake bottoms are, therefore, presumably very leaky; 3d, nearly all active volcanoes are situated either within, or in the neighborhood of, large bodies of water; 4th, volcanoes near the sea often deliver salts which may reasonably be supposed to be the same as those contained in the ocean; 5th, the analogy of geysers gives us a series of phenomena which seem to be, in many respects, quite parallel, and which have been satisfactorily explained in a similar way.

To this view of the origin and causation of volcanic activity there are some objections. There is difficulty in understanding how water obtains access to hot magmas. No doubt the rocks are full of fissures, but we cannot, by any means, confidently infer that these fissures extend sufficiently deep to afford free or even capillary passages to melted magmas beneath. We should more legitimately infer that the heat increases gradually with the depth. At a depth of a few miles the rocks presumably have a temperature which, though high, is still below fusion, and at such temperatures it is well known that all the siliceous or rocky materials we are acquainted with are viscous. Remembering the immense statical pressure due to a thickness of a single mile of rocks, all fissures at such depths would be closed, as if the rocks were wax or butter.

2d. Although the contact of cold water with intensely hot masses will surely produce a violent explosion, we are not at liberty to admit offhand that cold water does obtain such contact in the volcanoes. On the contrary, as it penetrates it takes up the heat of the rocks through which it passes. But water is believed by all physicists to have what is technically termed a critical temperature, *i. e.*, a temperature at which it can exist only in the form of vapor however great the pressure, and this temperature is computed theoretically to be about 772° F., which is far below that of melted rock. If therefore, water could reach the liquid lavas below, it would reach them only in the form of vapor. There is indeed no difficulty in supposing that the vapor of water may, under great statical pressure, be forced into the rocks, passing between inter-molecular spaces. This is but one aspect of the phenomena of the diffusion and occlusion of gases in solids, and we know that water-vapor in large quantities is readily occluded by lava. But this is evidently no explanation of the explosive action. It is in the broadest possible contrast with the gross conception of the sudden access of cold water to hot bodies. The presumption is, under the process here suggested, that the vapor of water might penetrate slowly into regions of great heat until the hot magmas were saturated, and then the process would come to a standstill. But there would be no volcano in this case, for the supposed condition is evidently statical and stable. For the pressure which is supposed to force the vapor in is that due to the hydrostatic pressure of a column of water. The pressure which keeps it from blowing out is that due to an equally high or even higher column of rock, the density of which is at least two and a half times greater.

3d. The analogy of the geyser thus fails to become a true homology, or an epitome of the volcano. For the geyser is due to the access of cold water to a cavity walled by hot rocks and its vaporization; the volcano, if due to the penetration of water, is due to penetration in the form of vapor in the first instance; and the difference is radical.

4th. The proximity of volcanoes to large bodies of water does not necessarily imply a logical and causal relation, and is not necessarily the true law of distribution. Another and perhaps a more rational law of distribution may be given. As a matter of fact all active volcanoes are not situated near seas or lakes, though in truth the exceptions are at the present time few, as for instance, Sangay, in the eastern Cordilleras of Peru, and the volcanoes of Central Asia. It seems as if Darwin had acutely divined the true association, viz: that volcanoes are situated in areas which are undergoing elevation. So far as we know this rule is without exception, but there are many cases where the verification of the elevation is wanting. So far, however, as the test has hitherto been applied it has approved the rule. This is especially conspicuous in the western half of our own country when applied to the late Tertiary and Post Tertiary volcanoes, and it is true, so far as known, of the Andes, Java, Phillippines, and Mediterranean, and I have recently been able to verify it in the case of the Hawaiian volcanoes. It happens that elevations, as well as subsidences, are much more frequent and extensive near coast lines than in continental interiors, whence the proximity of volcanoes to the sea becomes a secondary rather than a primary relation. But elevations also occur in continental interiors, though less frequently. And when they do occur, we find associated phenomena of volcanism as abundant and forcible as in littoral regions. This has been the case in the great Tertiary elevation of the Rocky Mountains, of the Alps, and of the Himalayan plateau. Darwin's law of the distribution of volcanoes is as thoroughly sustained by geological history as by modern instances;

while the other law, though largely predominant at the present period, shows a few conspicuous failures at the present time, but a very large number of them in times past.

Another hypothesis to account for volcanic energy supposes the interior of the earth to consist of unoxidized elements, which gradually become oxidized by the penetration of oxygen from the atmosphere.

The objections to this hypothesis are as follows: On the assumption that the earth acquires no oxygen from space, the primitive atmosphere would have been many thousand times greater than at present; but the geological record argues strongly in favor of an atmosphere which may indeed have varied in quantity and composition, but nowhere near so greatly as the hypothesis implies. Any such extravagant difference would have recorded itself legibly in the strata. Furthermore, on this view, the end of all volcanic activity is close at hand. Only three pounds of oxygen to the square inch of terrestrial surface are left. A few hundred or thousand centuries and the last volcanic beacon is extinguished, and with it all organic life.

But suppose the earth gathers up oxygen in its march through space. This may be true, but we can make any supposition on this point which pleases our fancy and feel sure that no prudent scientific man will dispute it.

A third hypothesis is that of the late Robert Mallet, which assumes the earth to be contracting interiorly by a secular loss of primitive heat. As the interior cools and shrinks, the external shell is crushed and crumpled together, and this mechanical crushing is a sufficient source of heat.

To this hypothesis there are many answers. The most direct one is that the very facts which are relied upon to prove that there is any interior cooling at all now going on also prove that the amount hitherto has been excedingly small, and has been limited as yet to a thin external shell, not exceeding 150 miles in thickness, while the great interior is about as hot as ever; but, by the terms of the hypothesis, if the interior has not cooled there has been no interior contraction. The hypothesis is refuted by taking its own premises and pushing them to their inevitable conclusions.

There is a fourth hypothesis, which cuts the Gordian knot instead of untying it. It assumes, as the result of causes unexplained, heat is generated locally within the earth, and such local movements of heat are the cause of volcanism. This is an arbitrary postulate, which, by its own terms, precludes discussion. Nevertheless it is the one which I believe agrees best, and perhaps perfectly, with observed facts. It undoubtedly sweeps away the difficulties which encumber all other hypotheses, but unfortunately it is an appeal to mystery, and therefore substitutes a single difficulty as great as, if not greater, than all the other difficulties put together.

There is a fifth hypothesis, which takes account of the fact that many bodies which are solid under great pressure are immediately liquefied when the pressure is removed, heat being neither lost nor gained. The removal of pressure by denudation of the surface above the seat of lavas may thus determine volcanic action. The reply to this is that volcanoes do not always, nor even generally, occur where such denudation and consequent relief of pressure, are in progress. The true law of the distribution of volcanoes appears to be the one given by the late Charles Darwin, viz., that they occur in areas which are undergoing elevation.

There are several broad facts, or categories of facts, which a true theory of the volcano must cover, and which will be recited briefly.

1. Lavas, in their subterranean seat, could not possibly have been in a highly elastic explosive condition from the earliest epochs of the earth's evolution, and only waiting a convenient season to break forth. We have no alternative but to regard them as being inert and inexplosive in their primitive condition, and as having acquired explosive energy just before the epoch of eruption. To assume that they have always been in the condition they present while pouring forth, and that the opening of a fissure has been the accident which determined the eruption, is reasoning in a circle. It is the energy of the lavas which causes the fissure, and not the fissure which causes the lavas to extrude. The lavas extrude themselves by virtue of their acquired elastic force. The theory must explain how materials which antecedently were inert, passive, incapable of eruption, may become active, dynamical, eruptible.

2. Another broad fact, closely related to the foregoing, is the intermittent action of volcanoes. These vents do not discharge all their available products at once, but by repeated spasms of activity, separated by longer intervals of repose. If these fiery explosive liquids had lain so long in the earth, chock-full of energy and only awaiting the opening of a passage-way, how happens it that when

a vent *is* once opened they do not all rush forth at once, and continue to outpour until the reservoir is completely exhausted, and why does not the vent thereafter close up forever? In a word, why should a volcano dole out its products in driblets, instead of sending forth one stupendous belch, equal to all the driblets combined? The answer here proposed is that it is because lavas, in their primitive condition, do not have sufficient potential energy, in the form of elastic force, to break open the covering which keeps them in; but they gradually acquire that energy in a portion of the reservoirs at a time, and when a sufficient portion of them has acquired it the covering is ruptured, and the whole of this energetic portion is extravasated. The vent then closes, and the process is repeated upon a second installment. The agency which thus progressively develops this force is the missing factor, and when we discover it we shall discover the secret of the volcano.

The third general fact to be taken account of is the enormous quantity of heat given off by volcanoes through long periods of time without any sign of exhaustion. The quantity of heat brought up by the lavas themselves is but a fraction of the whole amount dissipated. Kilauea wastes many times more heat by quiet radiation from the surfaces of its lava lakes and by steaming and by numberless modes of escape than by actual eruption of lavas. Mauna Loa also dissipates the greater part of its heat in the same way, and the same fact is wholly or partially true of all other active or intermittent volcanoes. And yet for very long periods, for thousands of centuries, these great volcanoes show no sign of heat-exhaustion; on the contrary, such indications as we have suggest the conclusion that the earth beneath them is hotter than before.

A fourth general fact is that volcanoes are located in areas which have recently been or are now undergoing elevation.

All these facts suggest the action of some cause generating heat within the earth. This cause, if such it be, is for the present wholly mysterious and unknown.

Mr. Powell, referring to the relation between volcanic eruption and elevation, said that the typical, secular sequence of geologic events was, first, elevation, resulting in, second, degredation, accompanied by, third, extravasation, followed sooner or later by, fourth, subsidence, resulting in, fifth, sedimentation. There are numerous regions in which this circle of events has been recorded, and in some places it has been repeated two or three times.

Mr. F. W. CLARKE suggested that the difficulty in the way of a chemical explanation of volcanic phenomena was due to our ignorance of chemical force under high pressures. Spring has lately shown that chemical union could be brought about by pressure alone. Hence, water coming in contact with molten rock matter in the interior of the earth might be prevented from dissociating. If, however, dissociation takes place, we may conceive that water may play the following part in volcanic explosions. Gradually filtering through the surface rocks to the hot lava, it would undergo slow decomposition, and great quantities of mixed oxygen and hydrogen would thus slowly accumulate. Now let a process of cooling begin. Soon the temperature at which oxygen and hydrogen unite would be reached, and explosive union would occur. This may account for volcanic explosions, at least in part. By such a process, potential energy is gradually stored up, to be later, suddenly or instantaneously, released. This hypothesis does not account for volcanic heat, but presupposes its existence.

Mr. WHITE, referring to Mr. Powell's remarks on the instability of continental areas, said that the prevalent doctrine of the permanence of oceans, and the gradual development of the continents, was not sustained by paleontology. Continents were needed somewhere to develop the land plants and land mammals which appeared during the emergence of the known continents.

Mr. HARKNESS pointed out that Mr. White was postulating unknown continents to support the Darwinian hypothesis, to which Mr. White assented.

Mr. POWELL added, that in detailing the great cycle of geologic events, he should have included metamorphism as a sixth term, resulting from burial by sediment; and Mr. DUTTON remarked that he had included this consideration in a paragraph contained in his written manuscript, but not read.

Mr. MCGEE made a communication on

THE DRAINAGE SYSTEM AND THE DISTRIBUTION OF THE LOESS OF EASTERN IOWA.

[Abstract.]

The most conspicuous geographic feature of eastern Iowa is the remarkable parallelism among its water-ways. Yet the region comprises two essentially distinct geologic tracts; and the coincidence in direction of drainage in these is fortuitous: 1. The Wisconsin Driftless Region so far extends into the northeastern corner of Iowa as to include all of the triangular area bounded on the southwest by the elevated Niagara escarpment extending from the extreme eastward projection of the state northwestwardly to the Minnesota line, fifty miles west of the Mississippi. Within this tract, the drainage was originally determined by general surface slope and by rock-structure, and the present topography, which is varied and picturesque, was developed by sub-aërial erosion. 2. Within the far more extensive tract formed by the glacial drift and its derivatives, the surface is a gently undulating plain, over which the general relief is inconspicuous, and the local topography faintly defined though singularly uniform and symmetric in character; and here the parallelism in drainage is prevalent and characteristic. There are, indeed, both local and general exceptions to this parallelism, which exemplify a variety of types of aberrant behavior of the streams; but while these impair the geographic symmetry of the drainage system, they add much more largely to its geologic significance. Putting together the instances of accordant, and neglecting the instances of aberrant extension of water-lines, a *normal direction of drainage* for the whole of the drift-formed tract might be empirically determined; which normal direction is represented by a symmetric series of slightly divergent and slightly curved lines, concave to the northeastward, radiating from a point north of the state in a general southeasterly direction, toward the Mississippi. Probably nowhere else on the surface of the globe does so symmetric a normal drainage system exist, and assuredly nowhere else does the sum of directions of stream-flow over so considerable an area present so few examples of departure from the normal.

The broader topographic features of eastern Iowa are dependant upon geologic structure. The dip of the rocks is to the southwest, and the outcrops of the several formations represented form successive approximately parallel zones (trending northwest and south-east), of which those of the Niagara and Hamilton are widest. Now the Niagara rocks resisted well the planation of the pre-quaternary eons, and their eastern margin is accordingly defined by a prominent escarpment varying from 1,000 to 1,350 feet in altitude, from which there is a steep northeasterly slope to the Mississippi, and a gentle inclination, corresponding to the dip of the strata, in the opposite direction. The Hamilton rocks, on the other hand, have so yielded to erosion that their area is topographically represented by a broad, shallow trough, of which the altitude is only from 600 to 1,000 feet, and of which the sides rise and culminate in the Niagara escarpment on the east and in the Mississippi-Missouri water-shed on the west. There is, however, a subordinate general topographic feature which is independent of geologic structure. A wide, gentle, indefinitely outlined depression extends directly across the great eastward projection (the "Cromwell's Nose") of Iowa and diagonally across the Upper Silurian, Devonian, and Carboniferous rocks alike, in the line of the general course of the Mississippi, from near the mouth of the Turkey to the mouth of the Iowa. It is manifestly of great antiquity.

Thus, in its general topography, eastern Iowa is characterized, primarily, by an elevated escarpment near its eastern border, by a broad depression intersecting its western portion diagonally, and by a general southwesterly slope extending over most of its area; and secondarily, by an indefinite ancient valley cutting off its eastern projection. And its general drainage system is almost absolutely independent of this general topography; for not only do the principal streams flow at right angles to the prevailing slope and cut through the elevated escarpment when it lies in their way, but, with the single exception of the Cedar, they preserve their courses directly across the ancient valley.

In their relation to minor topographic features the rivers of eastern Iowa conform to two diametrically opposite laws: 1. for two-thirds or three-fourths of their combined length they flow in the axes of the ill-defined, shallow valleys which characterize the drift-plain; and, 2, for the remaining portion of their courses they flow in narrow gorges which they have excavated for themselves in the axes of the elongated ridges that constitute the leading features in the local topography of the region. Moreover, they have in many instances, at the same time gone out of their direct courses, and deserted valleys already prepared for them, to attain the anomalous positions assumed under the second law of association. And let it be noted that in every such case the gorges have demonstrably been carved by the streams themselves through the quaternary and older formations alike; that the pre-existent valleys which they avoided have not been appreciably eroded since the quaternary; and that there has been no localized orographic movement in the region since long antecedent to the quaternary.

The principal tributaries entering the rivers from the right similarly conform to two antagonistic laws in their relation to topography: 1. Most of them flow throughout their courses in directions coincident with local and general slopes, and avoid elevations in their vicinity; and, 2. Many of them originate with directions approaching those normal to their localities, but curve more and more to the left toward their mouths, until they flow directly against the general slope, and enter the rivers at large angles; and all such streams have high north banks which they closely hug, and low south banks which they avoid.

So the drainage system of eastern Iowa is essentially independent of the more general topographic features, though affected by local topography; and the relations of the waterways to local topography are largely anomalous, and without parallel elsewhere.

Though essentially continuous stratigraphically, and of unquestionable genetic unity, the loess of eastern Iowa is variable in many characters, and may be separated into three geographic divisions; viz: 1, the Driftless Region division; 2, the Riparian division; and, 3, the Southern division. That of the first division forms the surface throughout the Driftless Region, as it exists in Iowa, and everywhere overlaps the eastern border of the drift; it is generally rather coarse, heterogeneous, and non-calcareous, and yields depauperate fossils of characteristic species; it reposes upon or graduates into a thin stratum of water-worn erratic materials, which, in turn, rests upon either the residuary clays of the Driftless Region or the margin of the drift-sheet; its western border is exceedingly sinuous, affects the greatest altitudes, and invariably overlooks the contiguous drift-plain; and, in capping the elevated Niagara escarpment, it forms the highest land within hundreds of miles, except in northerly directions. The loess of the Riparian division occurs chiefly in the elongated ridges so common and so intimately associated with the waterways in eastern-central Iowa; it is often fossiliferous, and its characters are generally typical; it usually graduates downward into stratified sands or gravels, which may or may not merge into drift; and it invariably seeks the highest summits in the region;—for the ridges in which the rivers have carved their cañons are always loess-topped; wherever streams avoid low-lying valleys for high-lying plateaus, the plateaus are of loess exteriorly; and the high northern banks of the aberrant tributaries are generally loess-capped. The loess of the Southern division prevails over southeastern Iowa; it abounds in characteristic fossils (which may or may not be depauperate), in loess-kindchen, and in calcareous tubes; it is fine, homogeneous, and vertically cleft; it generally graduates into the subjacent drift so imperceptably that neither geographic nor stratigraphic separation of the formations, by other than a purely arbitrary line, is possible; and it occurs indiscriminately at all levels.

So, in its distribution, the loess of eastern Iowa is intimately connected with the Driftless Region, with the drainage, and with the topographic configuration; but in its disposition to seek the greatest altitudes in the north, and to merge into the drift in the south, its behavior is as anomalous as is that of the rivers of the same region.

Mr. Powell remarked that these peculiarities of drainage were different from those observed in the drainage systems of mountain regions and demanded a different explanation, which was not yet forthcoming. It was probable, however, that not enough allowance was made for the differential effects of general degradation subsequent to the determination of the drainage.

Mr. Gilbert, after defining antecedent and super-imposed drainage, said that Mr. McGee's description definitely negatived the hypothesis of antecedent drainage, and rendered the hypothesis of super-imposed drainage in the ordinary sense equally untenable. The most plausible alternative is the hypothesis suggested by Mr. McGee in one of his earlier papers, that the drainage was super-imposed by the ice-sheet, the distribution of loess having been determined at the same time and by the same causes.

Mr. White regretted that Mr. McGee's special investigations did not include the portion of Iowa draining to the Missouri. The details of drainage in that region are equally interesting, and, in his opinion, do not admit of the explanation mentioned by Mr. Gilbert. The direction of the rivers diverges at right angles from that of the Mississippi tributaries, and their valleys are excavated from loess except along their upper courses.

Mr. Powell said that on the Illinois side of the Mississippi River many of the features described in the paper are repeated. The loess hills follow the river courses, and in the opposite directions overlook plains. The explanation of the phenomena is problematic, but the theory advocated by Mr. Gilbert does not appear sufficient.

241ST MEETING. NOVEMBER 24, 1883.

Vice-President BILLINGS in the Chair.

Fifty-three members and guests present.

It was announced by the Chair that the next meeting would be held in the Lecture Hall of the National Museum, that the members of the Anthropological and Biological Societies were invited to be present, and that the members of all three societies were requested to invite their friends.

Opportunity was afforded for the introduction of amendments to the Constitution, but none were offered.

Mr. C. D. WALCOTT made a communication on

THE CAMBRIAN SYSTEM IN THE UNITED STATES AND CANADA.

[Abstract.]

Defining the Paleozoic period as has been done by Geikie in his Text-Book of Geology, it will include all the older sedimentary formations containing organic remains, up to the top of the Permian. Upon the paleontologic evidence it may be divided into an "older and newer division, the former (from the base of the Cambrian to the top of the Silurian system) distinguished more especially by the abundance of its graptolitic, trilobitic, and brachiopodous fauna, and by the absence of vertebrate remains; the latter (from the top of the Silurian system to the top of the Permian system) by the number and variety of its fishes and amphibians, the disappearance of graptolites and trilobites, and the abundance of its cryptogamic terrestrial flora." The two divisions may be still further subdivided; the upper into the Carboniferous and Devonian, the lower into the Silurian above and the Cambrian beneath. It is the Cambrian division we now have to consider.

Stratigraphically it is difficult to fix any definite upper limit to the Cambrian system, owing to local causes having affected the conditions of sedimentation and consequent extinction or continuance of the fauna. Upon the evidence of the section in New York State on the western side of Lake Champlain, the Potsdam sandstone closes the period stratigraphically and paleontologically, the Calciferous formation forming little more than a closing deposit of the Potsdam; and the large Chazy fauna appearing suddenly in the overlying limestone is entirely distinct from that of the Potsdam. In central

Nevada the section passes through limestones marked by the presence of a typical Potsdam fauna and on up to one that has the general facies of that of the Trenton Lower Silurian fauna. Midway of these passage beds occur layers of rock that carry representatives of both the Cambrian and Silurian faunas. Above this band the Cambrian fauna gradually disappears, and below it soon predominates to the exclusion of the Silurian types. In this section we have an illustration of the gradual extinction of an older fauna as a new one is introduced, the sedimentation continuing and no physical disturbance occurring to change the conditions necessary for the presence of animal life. It is the ideal section uniting the faunas of two periods, and if we had the blanks filled in between all the groups, as the blank between the Potsdam and Chazy in New York is filled in by the Nevada section, the Paleozoic would be a record of continuous connected organic life from the base of the Cambrian to the summit of the Permian.

It is convenient for stratigraphic geologic work to separate the Paleozoic series into subdivisions, and, as this is almost necessarily done on paleontologic evidence, I would separate the Cambrian as one characterized by what Barrande has named the first fauna.* Applying this to the Nevada section already mentioned, the line between the Cambrian and Silurian would be drawn where the types of the second fauna begin to predominate. With this definition of the Cambrian system, the strata referred to it in the United States and Canada will be briefly noticed.

In the Grand Cañon of the Colorado the top of the Cambrian is the Tonto formation, a series of sandy calcareous strata 1,000 feet in thickness. The contained fauna is closely allied to that of the Potsdam sandstone and continues up to the summit of the formation, the overlying Devonian rocks resting directly above strata containing *Lingulepis*, *Iphidea*, *Conocephalites*, *Dicellocephalus*, etc. The Tonto rests uncomfortably on strata that were extensively eroded prior its deposition. This lower series comprises over 11,000 feet of unmetamorphosed shales, limestones, and sandstones, with 1,000 feet of interbedded lavas. It forms the Grand Cañon and Chu-ar′ groups of Powell and is characterized by the presence of a few fossils that enable us to refer it to the Cambrian but not to define its stratigraphic horizon. That is done on the evidence of the position it occupies with reference to the Tonto.

* The paleontologic evidence and discussion will appear in a future paper.

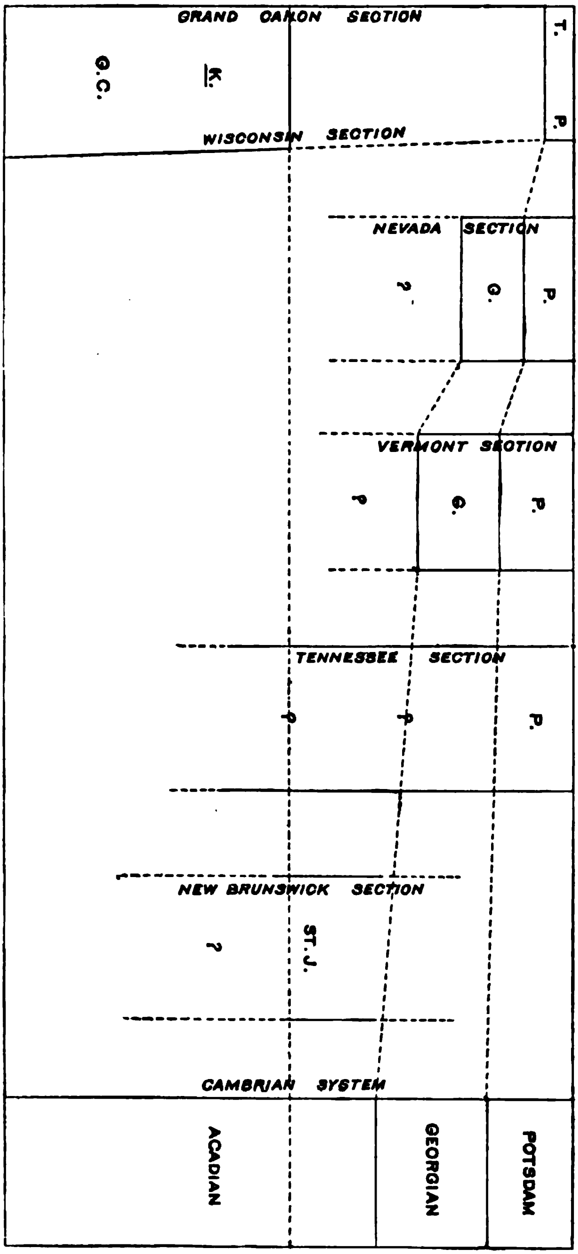
The relations of the Grand Cañon section are shown in the first column of the page of sections.

The Potsdam sandstone in Wisconsin occupies the same relative stratigraphic position as the Tonto formation, except that the break above the Tonto and between it and the Devonian is filled in by the Calciferous and other Silurian formations. As has already been said, the faunas of the Potsdam and Tonto are very much the same in general character. The Potsdam formation here overlies unconformably a series of strata that are directly comparable with the Grand Cañon and Chu-ar′ series. The Keweenawan series, according to Chamberlin, has about 10,000 feet of sedimentary strata distributed through 30,000 feet of eruptive rocks. In all this great mass no decisive evidence of organic life has been discovered, but knowing that the series is unconformably overlain by the Potsdam formation and that it in turn rests unconformably on the Archæan, as does the Grand Cañon series, we feel justified in correlating the Grand Cañon and Wisconsin sections and they are united in the first column of the page of sections.

The upper part of the Nevada section has already been mentioned. Below the Potsdam horizon there occurs a distinct fauna, characterized by a considerable development of the trilobitic genus *Olenellus*, a genus that in the embryonic development of several of its species proves that it is derived from the *Paradoxides* family and is consequently of later date. This section is readily correlated with that of the Georgian group of Vermont, as there we have the Potsdam sandstone above the Olenellus horizon, and in the downward section both stop at nearly the same relative horizon. The position of the Georgian formation in Nevada and Vermont, in relation to the Potsdam, leads to the view that it represents a portion of the period of erosion between the Tonto formation and the Grand Cañon series and also the Potsdam formation and the Keweenawan series.

The upper portion of the Tennessee Cambrian, the Knox shale, is correlated with the Potsdam sandstone, and so is the Knox sandstone. The Chilhowee sandstone and Ocoee conglomerate and slates cannot be directly connected with the Georgian horizon, since the paleontologic data are insufficient. From their position beneath the Knox shale with its Potsdam fauna they are extended downward past the Georgian and into the Paradoxidian or St. Johns horizon. Their total thickness (Geology of Tennessee, pp. 158, 159) is nearly 15,000 feet.

TYPICAL CAMBRIAN SECTIONS.



T. = Tonto. G. C. = Grand Cañon. P. = Potsdam. G. = Georgian. St. J. = St. Johns. K. = Keweenawan.
Maximum thickness of Cambrian = 50,000 + feet.

There is still another group, the St. Johns or Acadian, that occupies an horizon below the Georgian and may fill in a portion of the period of erosion between the pre-Potsdam and Keweenawan and the Tonto and Grand Cañon series, or it may represent some of the upper portions of the Grand Cañon and Keweenawan series. In the geologic sections it is placed beneath the Georgian and as above or passing down into the lower groups. For the present both it and the Paradoxidian argillites of Braintree must be left in doubt with regard to their relations to the lower Cambrian of Wisconsin and northern Arizona.

Of the Canadian survey sections, the one on the north side of the Straits of Belle Isle is most interesting as it gives the Georgian horizon, but unfortunately an interval of ten miles in width is occupied by the straits before the section is again continued. In this interval the Potsdam group is lost, but farther along the coast there occurs, below limestones referred to the Calciferous horizon, a mass of sandstone that may be assigned to the Potsdam formation—giving, in connection with the Olenellus or Georgian horizon, a section not unlike that of Central Nevada.

No other section that has been determined in the British Provinces throws much light on the stratigraphic succession of the Cambrian rocks. At Point Levis a curious mingling of the Cambrian and Silurian faunas has been said to occur, but this is rather to be attributed to error in the interpretation of the stratigraphy in a much disturbed area than to a break in the sequence of organic remains, elsewhere so uniform. I prefer to accept the interpretation given by M. Jules Marcou, who says (The Taconic and Lower Silurian Rocks of Vermont and Canada, Proc. Bos. Soc. Nat. Hist., Vol. VIII, p. 252, 1862,) that the primordial or Cambrian types are associated together and occur in a belt of limestone that contains no traces of the second or Silurian fauna.

The accompanying table of sections gives a general outline of the Cambrian. Numerous local sections of the Potsdam series are not mentioned, as they do not add materially to the general information in regard to the system in its vertical range.

The geographic range is great, extending as it does from Newfoundland to Montana on the northern line, and thence south to Nevada, Texas, and Alabama.

Mr. JOHN JAY KNOX made a communication on

THE DISTRIBUTION OF THE SURPLUS MONEY OF THE UNITED STATES AMONG THE STATES.

[Abstract.]

President Jackson, in his message to Congress in 1829, referred to the difficulty in adjusting the tariff, so that the revenues of the Government should be but slightly in excess of its expenditures. He considered the appropriation of money for internal improvements, by Congress, as unconstitutional, but suggested that, if the anticipated surplus in the Treasury should be distributed among the States, according to their ratio of representation, such improvements could then be made by the States themselves. If necessary it would be expedient to propose to the States an amendment to the Constitution, authorizing such legislation.

In his message for the following year he again suggested the same proposition.

The receipts from sales of public lands for the three years, 1834, 1835, and 1836, were $44,492,381—slightly less than the total receipts from this source for the thirty-eight years previous, from 1796 to 1834. On January 1, 1835, the country was virtually out of debt, and the receipts of the Government largely exceeded the previous estimates of the Secretary. The amount of surplus on January 1, 1835, was $8,892,858, and at the same date in 1836 $26,749,803. On January 1, 1837, it amounted to more than forty-two millions.

In 1834–5–6, the public money, which had heretofore been deposited in the Bank of the United States, was deposited in favorite State banks by order of General Jackson. The deposit of the revenues in these banks was followed by financial distress, and during the year 1834, and previous thereto, propositions were made in the public press for distribution of the surplus revenue among the States as a measure of relief. These propositions were first in the form of a distribution of the revenue from public land; then a distribution of the lands themselves; and finally a distribution of the surplus. During the session of 1835, a select committee was appointed in the Senate, which reported a resolution to amend the Constitution so that the money remaining in the Treasury at the end of each year, until the first of January, 1843, should annually be distributed among the States and Territories. Both General

Jackson and Secretary Woodbury were opposed to this proposition, as the withdrawal of public moneys would deprive the State banks of the deposits, and would be likely to increase the financial troubles. A bill to distribute the surplus was, however, introduced in the Senate, and passed by a vote of 25 to 20. It was evident that this bill could not pass the House, as a majority of its members considered the bill, in the form of a distribution, as unconstitutional. The friends of the measure in the Senate determined to change its form so as to remove the difficulty. A bill then pending in the Senate was so amended as to change the proposition for distribution to a proposition for deposit with the States, and in this form it passed the Senate, and subsequently the House by a large majority, 155 to 38.

This act of June 23, 1836, provided for the deposit with the treasurers of the several States of 37 millions ($37,468,859) in four instalments during the year 1837—the Secretary of the Treasury to receive certificates of deposit therefor signed by competent authority, in such form as he should prescribe, which certificates should express the usual legal obligation, and pledge the faith of the State for the safe keeping and repayment of the deposit, from time to time, whenever the same should be required. The first three installments were deposited. Before the last installment, payable on the 1st day of October, was transferred, a series of financial disasters culminated in the crisis of 1837, and there was no surplus to deposit. Further legislation was deemed necessary in this emergency, and an extra session of Congress was called by President Van Buren. During this session, on September 11, 1837, a bill was reported from the Finance Committee of the Senate, providing that the transfer of the fourth installment should be indefinitely postponed. The opposition to this bill was persistent, and there was a long debate, which was participated in by Webster, Clay, Calhoun, Buchanan, Benton, Silas Wright, Caleb Cushing, and others of the Senate; and in the House by Adams, Fillmore and Sibley of New York, Bell of Tennessee, Wise of Virginia, and many others.

A bill was finally passed, providing for the postponement of the deposit of the fourth installment until January 1, 1839. It passed the House by a vote of 119 to 117, and contained an amendment proposed by Mr. Buchanan, providing that the deposits should not be subject to the requisition of the Secretary of the Treasury, but should remain until called for by Congress. On the 1st of January, 1839, there were no funds in the Treasury available for the payment of the fourth installment, and since that date there has never been a surplus in the Treasury above the debts and estimated expenditures of the Government.

The amount of the three installments was $28,101,645, and the amount placed in the Treasury of each State has since been carried among "unavailable funds of the general Treasury," as may be seen by reference to the annual reports of the Treasurer of the United States.

The fourth installment, amounting to $9,367,215, has never been transferred or deposited, and recently the State of Virginia, through the action of its Legislature, and the State of Arkansas, through the action of its treasurer and one of its United States Senators, has applied to the Secretary of the Treasury for the payment of this last instalment.

It is generally believed that the moneys deposited by the Government with the different States were, for the most part, wasted or employed in works of internal improvement which were unnecessary. The data for a full investigation of this subject are not at hand, but it is known that the States of Massachusetts, Connecticut, New York, New Jersey, Pennsylvania, Delaware, Maryland, North Carolina, Illinois, Indiana, Kentucky, Ohio, and Missouri appropriated a considerable portion of the income from this fund to the support of public schools; and that in many of these States the income from the whole fund has been from the commencement, and still is, devoted to the education of the people.

A bill was introduced by Senator Logan, during the first session of the last Congress, providing that the entire income derived from the internal-revenue tax on the manufacture and sale of distilled spirits shall be appropriated and expended for the education of all children living in the United States, as shown by the census of 1880 and each succeeding census. The bill also provides that the States shall be required, before receiving the benefits of the act, to make school attendance obligatory upon all children between the ages of seven and twelve years, for at least six months in each year.

Mr. Alvord inquired as to the present status of the Smithsonian fund, amounting to about half a million of dollars, which was invested in the bonds of the State of Arkansas.

Mr. Knox said that the Government has assumed the Arkansas bonds formerly held by the Smithsonian Institution, and that the Government also held quite a large amount of the bonds of the States of Virginia and Arkansas in the Indian Trust Fund. If legislation should be obtained authorizing the payment of the fourth nstalment to these States, such legislation should provide that the payment be made in the bonds now held by the Government.

Mr. ALVORD said that the history of agricultural college grants was not thus far very encouraging. It would have been better if Congress had provided that the agricultural colleges should never be united with other colleges. The union was apt to lead to confusion and controversies, and lower the standard and prestige of both. Witness the case of Dartmouth College. In this reference Mr. MUSSEY concurred.

The Hon. HUGH MCCULLOUGH, being invited by the Chair to participate in the discussion, said that in Indiana the application of the money deposited by the United States had occasioned a long debate, which had resulted in its division. One half, by means of a system of commissioners, was loaned to individuals on land and mortgage; the other half was put into stock of the State Bank, with which the speaker was at that time connected. In a financial crisis the first half was practically lost, probably less than one-twentieth part being recovered; but the loss was fortunately made good by the bank stock, upon which dividends were regularly paid, and by which the investment was eventually doubled. Since the closing of the bank, this money has constituted the school fund of Indiana.

Mr. R. D. CUTTS made a communication on

THE ACTION OF THE INTERNATIONAL GEODETIC ASSOCIATION AS TO AN INITIAL MERIDIAN AND UNIVERSAL TIME.

[Abstract.]

The International Geodetic Association of Europe, formed for the purpose of connecting the systems of triangulation executed by the different States of Europe, and hence for the measurement of arcs, and for the discussion of all questions of science comprised within the term Geodesy, has been in active existence for many years. The meeting in 1882 was held at The Hague, and before adjournment it was decided that the seventh conference should meet at Rome, in October, 1883.

In the meantime, all governments in diplomatic relations with the United States were invited by the President, in accordance with the act of Congress, August 3, 1883, to send delegates to Washington for the purpose of fixing upon a meridian proper to be employed as a common zero of longitude and standard of time, reckoning throughout the globe. More than twenty of these countries had signified, before October last, their acceptance of the invitation, but these did not include many of the principal governments of Europe. The delay in forwarding their definitive replies was due to their desire to have the advice, before committing themselves, of the Eurpean Geodetic Association. Hence it was at the request of many of these governments that the Association took up the subject of the unification of longitudes, and of the introduction of a universal time.

So soon as it was decided to take such action, General Ibanez, of Spain, the then President of the Association, addressed a letter to the Superintendent of the Coast and Geodetic Survey, urging him in strong terms to send a delegate to the meeting at Rome. So short a notice was given, however, that the delegate selected had to start at once, reaching Rome only on the morning of the first day's session, October 15th.

After a full discussion of the different views presented, the following resolutions were almost unanimously passed on October 24th. It must be borne in mind that they are merely of an advisory character, sanctioned and urged, nevertheless, by the highest scientific authority. It is the function of the convention to be held at Washington next year to take official and decisive action on the subject in all its details.

Resolutions of the International Geodetic Commission in relation to the Unification of Longitudes and of Time.

The seventh general conference of the International Geodetic Association, held at Rome, and at which representatives of Great Britain, together with the directors of the principal astronomical and nautical almanacs, and a delegate from the Coast and Geodetic Survey of the United States, have taken part, after having deliberated upon the unification of longitude by the adoption of a single initial meridian, and upon the unification of time by the adoption of a universal hour, have agreed upon the following resolutions:

I. The unification of longitude and of time is desirable, as much in the interest of science as in that of navigation, of commerce, and of international communication. The scientific and practical utility of this reform far outweighs the sacrifice of labor and the difficulties of adaptation which it would entail. It should, therefore, be recommended to the Governments of all the States interested, to be organized and confirmed by an International Convention, to the end that hereafter one and the same system of longitudes shall be employed in all the institutes and geodetic bureaus, for the general geographic and hydrographic charts, as well as in the astronomical and nautical almanacs, with the exception of those made to preserve a local meridian, as, for instance, the almanacs for transits, or those which are needed to indicate the local time, such as the establishment of the port, &c.

II. Notwithstanding the great advantages which the general introduction of the decimal division of a quarter of the circle in the expressions of the geographical and geodetic co-ordinates, and in the corresponding time expressions, is destined to realize for the sciences and their applications, it is proper, through considerations eminently practical, to pass it by in considering the great measure of unification proposed in the first resolution.

However, with a view to satisfying, at the same time, very serious scientific considerations, the Conference recommends, on this occasion, the extension by the multiplication and perfection of the necessary tables, of the application of the decimal division of the quadrant, at least, for the great operations of numerical calculations, for which it presents incontestable advantages, even if it is wished to preserve the old sexagesimal division for observations, for charts, navigation, &c.

III. The Conference proposes to the Governments to select for the initial meridian that of Greenwich, defined by a point midway between the two pillars of the meridian instrument of the Observatory of Greenwich, for the reason that that meridian fulfils, as a point of departure for longitudes, all the conditions demanded by science; and because being at present the best known of all, it presents the greatest probability of being generally accepted.

IV. It is advisable to count all longitudes, starting from the meridian of Greenwich, in the direction from west to east only.

V. The Conference recognizes for certain scientific wants and for the internal service in the chief administrations of routes of communication, such as the railroads, steamship lines, telegraphic and post routes, the utility of adopting a universal time, along with local or national time, which will necessarily continue to be employed in civil life.

VI. The Conference recommends, as the point of departure of universal time and of cosmopolitan date, the mean noon of Greenwich which coincides with the instant of midnight, or with the commencement of the civil day, under the meridian situated 12 hours or 180 degrees from Greenwich.

It is agreed to count the universal time from 0^h to 24^h.

VII. It is desirable that the States which, for the purpose of adopting the unification of longitudes and of time, find it necessary to change their meridians, should introduce the new system of longitudes and of hours as soon as possible.

It is equally advisable that the new system should be introduced without delay in teaching.

VIII. The Conference hopes that if the entire world should agree upon the unification of longitudes and of time by accepting the meridian of Greenwich as the point of departure, Great Britian will find in this fact an additional motive to make, on its part, a new step in favor of the unification of weights and measures, by acceding to the *Convention du Mètre* of the 20th May, 1875.

IX. These resolutions will be brought to the knowledge of the Governments and recommended to their favorable consideration, with the expression of a hope that an International Convention, confirming the unification of longitudes and of time, shall be concluded as soon as possible, by means of a special conference, such as the Government of the United States has proposed.

Mr. HILGARD said that while the report of the Association did not conform in some of its details to the desires and interests of this country, nevertheless our principal object had been gained by the endorsement of the Association for the International Conference on the subject of standard time, to be held in Washington.

The selection of the meridian of Greenwich as the starting point for longitudes, was more convenient for us than for Europeans; Europeans alone are liable to the confusion arising from the numerical identity of meridians east and west of Greenwich. It will be impossible, however, for us to agree to the rule which counts all longitudes from west to east.

Mr. ELLIOTT opposed the establishment of noon as the initial hour of the day. It seemed to be proposed in the interest of astronomers, who work at night, and would not be submitted to by the people at large.

He exhibited a map showing a grouping of the railroads of the country under the recently adopted time schedule.

Mr. CUTTS said that the resolutions of the Geodetic Association do not appertain to civil time. The "universal time" they advocate is for the use only of astronomers and great transportation corporations.

Other remarks were made by Mr. NEWCOMB.

242D MEETING. DECEMBER 8, 1883.

By permission of the Secretary of the Smithsonian Institution, the Society occupied for the evening the Lecture Hall of the National Museum.

The President called Vice-President MALLERY to the Chair.

There were present about three hundred members and guests.

By invitation, the Presidents of the Biological and Anthropological Societies occupied seats on the platform.

The President of the Society, Mr. J. W. POWELL, delivered the annual address, taking for his subject

THE THREE METHODS OF EVOLUTION.

[The address is printed on pages XXVII–LII, *ante*.]

The Chair invited the members of the Society and their friends to remain for a period after adjournment, for the purpose of social intercourse.

The Society then adjourned.

243D MEETING. DECEMBER 22, 1883.

THE THIRTEENTH ANNUAL MEETING.

The President in the chair.

Thirty-four members present.

The minutes of the 226th, 241st, and 242d meetings were read.

The Chair announced the death, since the last meeting, of General R. D. CUTTS.

The Chair announced the election to membership of Messrs. ROBERT SIMPSON WOODWARD, DANIEL ELMER SALMON, and JOHN MILLS BROWNE.

The Secretary's report on the membership of the Society was read. During the year the Society received seventeen new members, lost eight by death, and lost three by resignation.

The Treasurer not being present, the Chair appointed Mr. Henry Farquhar Treasurer *pro tempore.*

The officers for the ensuing year were then elected by ballot. (The list is printed on page xv.)

On motion of Mr. JENKINS, the vote for President was made unanimous.

The Chair appointed Messrs. C. A. White, S. Newcomb, and H. C. Yarrow a committee to audit the annual report of the Treasurer.

The Society then adjourned.

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

MATHEMATICAL SECTION.

STANDING RULES

OF THE

MATHEMATICAL SECTION.

Adopted March 24, 1883.

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1. The object of this Section is the consideration and discussion of papers relating to pure or applied mathematics.

2. The special officers of the Section shall be a Chairman and a Secretary, who shall be elected at the first meeting of the Section in each year, and discharge the duties usually attaching to those offices.

3. To bring a paper regularly before the Section it must be submitted to the Standing Committee on Communications for the stated meetings of the Society, with the statement that it is for the Mathematical Section.

4. Meetings shall be called by the Standing Committee on Communications whenever the extent or importance of the papers submitted and approved appear to justify it.

5. All members of the Philosophical Society who wish to do so may take part in the meetings of this Section.

6. To every member who shall have notified the Secretary of the General Committee of his desire to receive them, announcements of the meetings of the Section shall be sent by mail.

7. The Section shall have power to adopt such rules of procedure as it may find expedient.

LIST OF MEMBERS

WHO RECEIVE ANNOUNCEMENT OF MEETINGS OF THE

MATHEMATICAL SECTION.

Abbe, C.
Alvord, B.
Avery, R. S.
Babcock, O. E.
Baker, M.
Bates, H. H.
Billings, J. S.
Burgess, E. S.
Christie, A. S.
Coffin, J. H. C.
DeLand, T. L.
Doolittle, M. H.
Eastman, J. R.
Elliott, E. B.
Farquhar, H.
Flint, A. S.
Gilbert, G. K.
Newcomb, S.
Gore, J. H.
Green, B. R.
Hall, A.
Harkness, W.
Hazen, H. A.
Hilgard, J. E.
Hill, G. W.
King, A. F. A.
Kummell, C. H.
Lefavour, E. B.
Peirce, C. S.
Ritter, W. F. M'K.
Smiley, C. W.
Taylor, W. B.
Upton, W. W.
Walling, H. F.
Winlock, W. C.

INAUGURAL ADDRESS

OF THE

CHAIRMAN OF THE MATHEMATICAL SECTION,

BY ASAPH HALL.

GENTLEMEN OF THE MATHEMATICAL SECTION:

I thank you for the honor you have conferred on me by my election as Chairman of this Section, and the best return that I can make is to do my utmost to render our meetings as interesting and successful as possible.

Although my duties have been such that I have not been able to take a very active part in the proceedings of the Philosophical Society, it is easy to understand how a need has been felt for a more full and frequent discussion of mathematical questions. Mathematics has indeed been called the queen of the sciences, but the rigor and dryness of its methods make it distasteful to many. The fact seems to be that as any branch of knowledge advances and finally is reduced to law, it loses in a large degree its attractiveness and popularity. Then, it is only with the indefinite outlines and the obscure boundaries of this science that most people like to deal; and this may be natural and right, since nearly all advancement originates in speculation and doubt, which lead to investigation, and which, by a variety of motives, spur men on to labor. But the science of mathematics, though old, is yet young and vigorous. We have now six journals of the highest rank, which are devoted almost exclusively to pure mathematics—two in Germany, two in France, one in England, and, I am glad to say, one in our own country. These journals are devoted to the discussion of the highest conceptions of space and number, treating chiefly of the laws and forms of analytical expressions, and generally they touch lightly on any practical application of the science. Such discussions prepare the way, however, for better and more general practical methods, and in our own country they have, I think, another value. For one, I can hardly accept the doctrine, advocated in some quarters, that the American scientific man of the future should be distinguished by his facility in getting a patent on his discovery, in forming joint stock companies and watering stock, and in suddenly becoming rich at the expense of his fellow-men. Such a career may be a natural result of our present system of sociology, but it does not seem to be in harmony with scientific thought and research, and our social need is for men of a different character. Far nobler is the life of one who devotes himself to the study of the most abstract forms of science; winning for us, if haply he may, another forward step up the hill of knowledge.

But when we come to the field of applied mathematics we soon learn how necessary are the studies of the pure mathematician. Nearly all the researches in natural philosophy, where the action of forces is concerned, require the formation and solution of differential equations, and hence the theory of such equations becomes important, and in some cases almost essential, for the advancement of physical investigations. It is not, of course, to be supposed that experiment and observation are to be done away with or neglected, or that mere skill in differentiating, integrating, and solving equations can supply the place of correct thinking. In fact, we may be sure that Leibnitz was mistaken when he declared that the invention of the differential calculus had made known that royal road to knowledge for which the king had inquired in vain of Euclid. But still it remains true that this calculus forms the most powerful engine we have for the solution of questions in natural philosophy. It enables us to adopt the old maxim, "*divide et impera.*" If we can reduce the problem to its elements, and can form its true differential equation, the rest of the work is purely mathematical. Unfortunately, the differential equations that occur in the problems of nature are very different from those given in our text-books, and their exact solution is in most cases impossible. Here we must rely chiefly on that happy device of the variation of constants, by means of which the solution of simpler forms is extended to the more complex.

One of the great advantages of putting a question in a mathematical form is the precision with which it can be stated. If we are right, the truth of our assertion will be the sooner acknowledged, and if we are wrong, our error can be the more easily detected. Frequently it has seemed to me that disputes would be avoided in the meetings of our scientific societies if men would take the trouble to put their assertion into a formula and write it on the blackboard;

and certainly there would be a clearness and meaning that are so often wanting. Thus, if any one asserts that when a planet comes to its perihelion it ought to fall into the sun, the law of gravitation being true, he is not worth listening to unless he will put his assertion into a formula; and when he is able to do this he will probably find out his own error. There will be so much gain by simply reducing the problem to its elements and giving it a correct form. Again, where scientific statements *may be* true, there will be a gain in giving them, when possible, a mathematical expression. Thus, when we are told that the fixed star 1830 Groombridge is running away, disobedient to the law of gravitation, how much better it would be if we could see on the blackboard the mathematical proof of this assertion, so that we could judge for ourselves on what assumption it is based. The subject of impulsive forces is one that we hear disputes about in our own society, and it seems to be a fair field for a mathematical exposition. How often do we see such phrases as "energy," "potential energy," "kinetic energy," "conservation of energy," "work," "virial," &c. Could not some one of our members give us a clear account of these terms, show us how they are connected with the general equations of mechanics, what new ideas they contain, and on what limitations they may be based? As the application of mathematics is extended, sounding phrases are sure to come into use, and it is well to test them and know what they mean.

In the discussions of this Section, while all are invited to be critical, I trust that we shall all be kind and good tempered. We come together for discussion and mutual improvement, and while error is not to be spared we must be charitable to each other's faults.

BULLETIN

OF THE

MATHEMATICAL SECTION.

A communication signed by Mr. J. E. Hilgard and nineteen other members of the Philosophical Society, asking that a Section in Mathematical Science be formed, as provided in Paragraph 6 of the Standing Rules of the Society, was presented to the General Committee at its regular meeting January 27, 1883. The proposition was agreed to, and Mr. Hilgard was empowered to call a special meeting for the purpose of organizing such a section; the call being extended to all members of the Society.

1st Preliminary Meeting. February 17, 1883.

Twelve members met in the library of the Army Medical Museum, in answer to the first call.

Mr. Hilgard not being present, Mr. E. B. Elliott was called to the Chair.

An informal discussion followed, which brought out a unanimous sentiment in favor of forming the Section.

With some differences of opinion as to details, it was agreed to postpone formal action, and the meeting adjourned subject to call.

2d Preliminary Meeting. March 5, 1883.

Mr. Hilgard in the Chair.

Fifteen members present.

A plan of organization was adopted, and referred to the General Committee of the Society for consideration.

1st Regular Meeting. March 29, 1883.

Fourteen members present.

In the absence of Mr. Hilgard, who had presided over the meeting for organization, Mr. G. W. Hill was called to the Chair.

The standing rules for the government of the Section, as adopted at the last meeting of the General Committee of the Society, were read.

The Section then proceeded to elect officers for the year 1883. On motion of Mr. Winlock the rules of the Society at its Annual Meeting were followed.

Mr. Asaph Hall was chosen Chairman and Mr. H. Farquhar Secretary.

A letter from Mr. Marcus Baker, dated Los Angeles, Cal., was read by Mr. Christie. It expressed a strong interest in the Section, recommending that it should be conducted as nearly as possible on the plan devised by the late Prof. Henry for the Society itself, by which business and science are kept apart. A free use of pencil and paper at the meetings, and seats around a table, were further suggested. The letter closed by advocating the foundation of a new mathematical journal.

Mr. Christie then made a communication on

A QUASI GENERAL DIFFERENTIATION.

The paper was discussed by Messrs. Kummell, Elliott, Hill, and Doolittle. The author reserves it from publication to await further research.

A resolution was passed, requesting the committee in charge of the matter to call meetings of the Section on Wednesday evenings.

2d Meeting. April 11, 1883.

The Chairman, Mr. Hall, presided.

Present, ten members and two invited guests.

It was announced that the Editor of "Science" would publish brief reports of the meetings of the Section.

The Chairman read an inaugural address, [given in full on pp. 117 to 119 *ante.*]

Mr. C. H. KUMMELL then began a paper on

ALIGNMENT CURVES,

which was not finished at the time of adjournment.

3D MEETING. APRIL 26, 1883.

The Chairman presided.

Present, sixteen members and one invited guest.

Mr. KUMMELL completed his paper, begun at the second meeting, on

ALIGNMENT CURVES ON ANY SURFACE, WITH SPECIAL APPLICATION TO THE ELLIPSOID.

[Abstract.]

The attempt to put a number of points in line on a curved surface whose normals are supposed to be given (abstraction is made of deviations of the plumb-line and lateral refraction) gives rise to various curves, which I call alignment curves. There are two classes—alignment curves with two given termini and those with a starting point only. There are three distinct curves of the first class, viz.: 1. The normal section, if the surveyor directs his assistant to place staffs in line from one end of the line. 2. A curve described if the surveyor would align a point near him, then move up to this point, thence align another point, etc., until the terminus is reached. This process is that used in chaining, or more roughly by a pedestrian going towards a point, and is characterized by requiring only foresights. I call it *proörthode* (πρὸ, ὀρθὸς, ὁδός).* 3. A curve resulting if a backsight is also taken. This curve is therefore defined by the condition that the normal plane at any point of it which passes through one end also passes through the other. I call it *diorthode* (διὰ, ὀρθὸς, ὁδός), because it may be con-

* This and other names of curves were coined by my friend, Mr. Wm. R. Galt, of Norfolk, Va.

sidered straight *all through* at any of its points. This curve may be considered the ideal curve of a primary base line. Various names have been given to it when on the terrestrial spheroid. Dr. Bremiker, who appears to have first considered it (in his Studien ueber hoehere Geodæsie, 1869), proposed the name "Feldlinie"; that is, field line. He thinks it should be adopted as the geodetic line, because both linear and angular measurements conform to it. Clarke, Zachariæ, and Helmert have also mentioned it, the latter, however, only in a note, where he remarks that it deserves no consideration in geodesy.

To the second class belong two curves: 1. A curve described as follows: The surveyor at the starting point takes his directions from a staff at short distance and directs his assistant to place a staff in the prolongation. Repeating this operation from the first staff, from the second staff, etc., he describes a curve which is well known to be the shortest curve between any of its points. It is usually called the geodetic line. However, since this name would apply at least equally well to the three curves already considered, I propose the name *brachisthode* (βράχιστος). The properties of this curve need not be considered here, such mathematicians as Gauss, Hansen, Bessel, and others, having perfected its theory. Helmert, in his "Hoehere Geodæsie," makes this curve the basis of nearly all geodetic computations. The brachisthodic process on a plane evidently results in a straight line, and on a sphere in a great circle. If, on these surfaces, it is in starting directed to a distant point, that point will be reached (disregarding errors of observation). Not so on other curved surfaces; there, in general, the first element of the brachisthode is not in direction to any of its points at a finite distance. 2. The *loxodrome* being a curve which has a constant inclination to a given direction, may, perhaps, be mentioned as belonging to this class.

The general equations of the two-end curves on any surface may be developed as follows:

Let the equation of the surface be:

$$u = f(x, y, z) = 0 \tag{1}$$

then if (ξ, η, ζ) is any point in the normal at the surface point (x, y, z), we have its equations:

$$\frac{\xi - x}{\left(\frac{du}{dx}\right)} = \frac{\eta - y}{\left(\frac{du}{dy}\right)} = \frac{\zeta - z}{\left(\frac{du}{dz}\right)} \tag{2}$$

and the equation of a normal plane at the surface point (x, y, z) and passing through (x_2, y_2, z_2), (not necessarily a surface point, but considered so here), is:

$$0=\left[(\xi-x)\left(\frac{du}{dz}\right)-(\zeta-z)\left(\frac{du}{dx}\right)\right]\left[(y_2-y)\left(\frac{du}{dz}\right)-(z_2-z)\left(\frac{du}{dy}\right)\right]$$

$$-\left[(\eta-y)\left(\frac{du}{dz}\right)-(\zeta-z)\left(\frac{du}{dy}\right)\right]\left[(x_2-x)\left(\frac{du}{dz}\right)-(z_2-z)\left(\frac{du}{dx}\right)\right]$$

$$=[(y_2-y)(\xi-x)-(x_2-x)(\eta-y)]\left(\frac{du}{dz}\right)$$

$$+[(z_2-z)(\eta-y)-(y_2-y)(\zeta-z)]\left(\frac{du}{dx}\right)$$

$$+[(x_2-x)(\zeta-z)-(z_2-z)(\xi-x)]\left(\frac{du}{dy}\right) \qquad (3)$$

If in this we replace the surface point (x, y, z) by the surface point (x_1, y_1, z_1) and (ξ, η, ζ) by the surface point (x, y, z) we obtain:

$$0=[(y_2-y_1)(x-x_1)-(x_2-x_1)(y-y_1)]\left(\frac{du}{dz_1}\right)$$

$$+[(z_2-z_1)(y-y_1)-(y_2-y_1)(z-z_1)]\left(\frac{du}{dx_1}\right)$$

$$+[(x_2-x_1)(z-z_1)-(z_2-z_1)(x-x_1)]\left(\frac{du}{dy_1}\right) \qquad (4)$$

which, if combined with the equation of the surface, gives the normal section at (x_1, y_1, z_1) through (x_2, y_2, z_2).

If, however, we replace in (3) (ξ, η, ζ) by the surface point (x, y, z) we obtain:

$$0=[(y_2-y)(x_1-x)-(x_2-x)(y_1-y)]\left(\frac{du}{dz}\right)$$

$$+[(z_2-z)(y_1-y)-(y_2-y)(z_1-z)]\left(\frac{du}{dx}\right)$$

$$+[(x_2-x)(z_1-z)-(z_2-z)(x_1-x)]\left(\frac{du}{dy}\right) \qquad (5)$$

and this, combined with the equation of the surface, gives the diorthodic curve.

As we move along the diorthode, (5) may be considered a plane which turns about the chord (1, 2) as an axis, so as to be always normal to the surface. It follows that the normals at any point of the diorthede are constrained to pass through the chord. They will thus generate a ruled surface, whose equation is not (5) however.

The equation of this ruled surface is obtained by eliminating x, y, z from (1), (2), and (5). It is important to remark that the diorthode does not consist of parts which are diorthodes with respect to their termini, otherwise the normals would at the same time pass through two chords from the same point and the curve would be a plane curve. Dr. Bremiker had erroneously supposed that the diorthode was touched by the normal planes. This is only the case at the termini. He has been criticized by Dr. Bruns of Pulkowa and by Helmert, but neither critic has shown the existence of a curve possessing this property, namely, the proörthode, in which the normal plane at any of its points passes through the consecutive point and the forward terminus, but not in general through the starting point. If then in (5) we replace (x_1, y_1, z_1) by $(x + dx, y + dy, z + dz)$ we have:

$$
\begin{aligned}
0 &= [(y_2 - y)\,dx - (x_2 - x)\,dy]\left(\frac{du}{dz}\right) \\
&+ [(z_2 - z)\,dy - (y_2 - y)\,dz]\left(\frac{du}{dx}\right) \\
&+ [(x_2 - x)\,dz - (z_2 - z)\,dx]\left(\frac{du}{dy}\right) \\
&= \left[(y_2 - y)\left(\frac{du}{dz}\right) - (z_2 - z)\left(\frac{du}{dy}\right)\right]dx \\
&+ \left[(z_2 - z)\left(\frac{du}{dx}\right) - (x_2 - x)\left(\frac{du}{dz}\right)\right]dy \\
&+ \left[(x_2 - x)\left(\frac{du}{dy}\right) - (y_2 - y)\left(\frac{du}{dz}\right)\right]dz \qquad (6)
\end{aligned}
$$

By means of the equation of the surface (1) and its differential equation

$$0 = \left(\frac{du}{dx}\right) dx + \left(\frac{du}{dy}\right) dy + \left(\frac{du}{dz}\right) dz \qquad (7)$$

any one of the variables with its differential can be eliminated. The resulting differential equation being integrated so as to contain the starting point (x_1, y_1, z_1), will be the equation of a projection of the proörthode on a coördinate plane.

The proörthode being differently related to its ends, will be different forward and backward, while the diorthode is the same forward and backward.

The following diagram will illustrate the relative course of these curves:

Any surface of the second degree may be represented by

$$u = 0 = \left(\frac{x-a}{a}\right)^2 + \frac{y^2}{p} + \frac{z^2}{q} - a \qquad (8)$$

The origin is taken at one of its real vertices, so that $(a, 0, 0)$ is its centre. The equation of the diorthode is then by (5), if we write $x_2 - x_1 = \Delta x$; $y_2 - y_1 = \Delta y$; $z_2 - z_1 = \Delta z$,

$$\begin{aligned}
0 &= [(y_2 - y)(x_1 - x) - (x_2 - x)(y_1 - y)]\frac{z}{q} \\
&+ [(z_2 - z)(y_1 - y) - (y_2 - y)(z_1 - z)]\frac{x-a}{a} \\
&+ [(x_2 - x)(z_1 - z) - (z_2 - z)(x_1 - x)]\frac{y}{p} \\
&= (y_2 x_1 - y_1 x_2 + y\Delta x - x\Delta y)\frac{z}{q} \\
&+ (z_2 y_1 - z_1 y_2 + z\Delta y - y\Delta z)\frac{x-a}{a} \\
&+ (x_2 z_1 - x_1 z_2 + x\Delta z - z\Delta x)\frac{y}{p} \\
&= \Delta x\left(\frac{1}{q} - \frac{1}{p}\right) yz + \Delta y\left(\frac{1}{a} - \frac{1}{q}\right) xz + \Delta z\left(\frac{1}{p} - \frac{1}{a}\right) xy \\
&+ (z_2 y_1 - z_1 y_2)\frac{x-a}{a} \\
&+ (x_2 z_1 - x_1 z_2 + p\Delta z)\frac{y}{p} + (y_2 x_1 - y_1 x_2 - q\Delta y)\frac{z}{q} \qquad (9)
\end{aligned}$$

The equations of the chord (1, 2) may be written:

$$\frac{x_2 - x}{x_1 - x} = \frac{y_2 - y}{y_1 - y} = \frac{z_2 - z}{z_1 - z} \qquad (10)$$

Every point of the chord, therefore, satisfies (9), and since that represents a surface of the second degree, it must be a hyperboloid of one sheet, for this and its varieties are the only ruled surfaces of that order. In the general form (9) it has a center in finite space. It is then the elliptic hyperboloid; but if $a = p$ (or $a = q$ or $p = q$), it has its center at an infinite distance, and it is a parabolic hyperboloid. In this case the base surface becomes:

$$0 = \frac{(x-a)^2 + y^2}{a} + \frac{z^2}{q} - a \tag{11}$$

which is a surface of revolution of the second degree.

If $a = p = q$, then (9) becomes a plane and the base surface a sphere. (9) is evidently satisfied by the center $(a, 0, 0)$, therefore the intersecting surface always passes through the center of the base surface.

I consider now the ellipsoid:

$$0 = \frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} - 1 \tag{12}$$

We have then the intersecting surface of the diorthode:

$$0 = (z_2 y_1 - z_1 y_2 + z\Delta y - y\Delta z)\frac{x}{a^2} + (x_2 z_1 - x_1 z_2 + x\Delta z - z\Delta x)\frac{y}{b^2}$$

$$+ (y_2 x_1 - y_1 x_2 + y\Delta x - x\Delta y)\frac{z}{c^2} \tag{13}$$

Let $(0, y_x, z_x)$ be the point where the chord (1, 2) pierces the yz-plane
$(x_y, 0, z_y)$ " " " " " zx- "
$(x_z, y_z, 0)$ " " " " " xy- "
then we can easily verify the relations:

$$y_x = \frac{x_2 y_1 - x_1 y_2}{\Delta x}\ ;\ z_x = \frac{x_2 z_1 - x_1 z_2}{\Delta x} \tag{14$_x$}$$

$$z_y = \frac{y_2 z_1 - y_1 z_2}{\Delta y}\ ;\ x_y = \frac{y_2 x_1 - y_1 x_2}{\Delta y} \tag{14$_y$}$$

$$x_z = \frac{z_2 x_1 - z_1 x_2}{\Delta z}\ ;\ y_z = \frac{z_2 y_1 - z_1 y_2}{\Delta z} \tag{14$_z$}$$

and if we assume:

$$\alpha_b^2 = 1 - \frac{a^2}{b^2}\ ;\ \alpha_c^2 = 1 - \frac{a^2}{c^2} \tag{15$_a$}$$

$$\beta_c^2 = 1 - \frac{b^2}{c^2}\ ;\ \beta_a^2 = 1 - \frac{b^2}{a^2} \tag{15$_b$}$$

$$\gamma_a^2 = 1 - \frac{c^2}{a^2}\ ;\ \gamma_b^2 = 1 - \frac{c^2}{b^2} \tag{15$_c$}$$

(13) will take either of the following equivalent forms:

$$0 = \Delta z\,(y_z - \alpha_b^2 y)\,\frac{x}{a^2} + \Delta x\,(z_x - \beta_c^2 z)\frac{y}{b^2} + \Delta y\,(x_y - \gamma_a^2 x)\frac{z}{c^2} \quad (13^1)$$

$$0 = \Delta y\,(z_y - \alpha_c^2 z)\,\frac{x}{a^2} + \Delta z\,(x_z - \beta_a^2 x)\frac{y}{b^2} + \Delta x\,(y_x - \gamma_b^2 y)\frac{z}{c^2} \quad (13^{11})$$

The following relations will be much referred to:

$$0 = \frac{y_z}{\Delta y} + \frac{z_y}{\Delta z} = \frac{z_x}{\Delta z} + \frac{x_z}{\Delta x} = \frac{x_y}{\Delta x} + \frac{y_x}{\Delta y} \quad (16)$$

$$\frac{x_y - x_z}{x_y} = \frac{y_z}{y_x} = \frac{z_y}{z_y - z_x}\,;\quad \frac{y_z - y_x}{y_z} = \frac{z_x}{z_z} = \frac{x_z}{x_z - x_y}\,;$$

$$\frac{z_x - z_y}{z_x} = \frac{x_y}{x_z} = \frac{y_x}{y_x - y_z} \quad (17)$$

$$0 = x_y\, y_z\, z_x + y_x\, z_y\, x_z \quad (18)$$

Replacing in these Δx, Δy, Δz; y_x, z_y, x_z; z_x, x_y, y_z

$$\text{by } \frac{1}{a^2},\ \frac{1}{b^2},\ \frac{1}{c^2}\,;\ \alpha_b^2, \beta_c^2, \gamma_a^2\,;\ \alpha_c^2, \beta_a^2, \gamma_b^2 \quad (19)$$

$$\text{we have: } 0 = \frac{\gamma_b^2}{c^2} + \frac{\beta_c^2}{b^2} = \frac{\alpha_c^2}{a^2} + \frac{\gamma_a^2}{c^2} = \frac{\beta_a^2}{b^2} + \frac{\alpha_b^2}{a^2} \quad (16^1)$$

$$\frac{\beta_a^2 - \gamma_a^2}{\beta_a^2} = \frac{\gamma_b^2}{\alpha_b^2} = \frac{\beta_c^2}{\beta_c^2 - \alpha_c^2}\,;\quad \frac{\gamma_b^2 - \alpha_b^2}{\gamma_b^2} = \frac{\alpha_c^2}{\beta_c^2} = \frac{\gamma_a^2}{\gamma_a^2 - \beta_a^2}\,;$$

$$\frac{\alpha_c^2 - \beta_c^2}{\alpha_c^2} = \frac{\beta_a^2}{\gamma_a^2} = \frac{\alpha_b^2}{\alpha_b^2 - \gamma_b^2} \quad (17^1)$$

$$0 = \beta_a^2 \gamma_b^2 \alpha_c^2 + \alpha_b^2 \beta_c^2 \gamma_a^2 \quad (18^1)$$

and these relations also will be found correct.

Because in the equation of the diorthodic surface the terms in x^2, y^2, z^2 are wanting, there must be lines, perpendicular to the co-ordinate planes, lying wholly in the surface. To determine those perpendicular to the xy-plane, I place $= 0$ the term in (13^1) dependent on z and that in (13^{11}) independent of z, or

$$0 = -\,y\Delta x\,\frac{\beta_c^2}{b^2} + \frac{\Delta y}{c^2}(x_y - \gamma_a^2 x)$$

$$= -\,\frac{\gamma_b^2}{\gamma_a^2}\cdot\frac{x_y}{y_x}\,y + \left(\frac{x_y}{\gamma_a^2} - x\right) \text{ by (16) and } (16^1)$$

$$0 = \Delta y z_y\,\frac{x}{a^2} + \Delta z\,(x_z - \beta_a^2 x)\frac{y}{b^2}$$

$$= \frac{y_z}{\alpha_b^2}\,x + \left(\frac{x_z}{\beta_a^2} - x\right) y \text{ by (16) and } (16^1)$$

Substituting the value of y from the first into the second equation we have:

$$0 = \frac{y_z}{\alpha_b^2}x + \frac{\gamma_a^2}{\gamma_b^2}\cdot\frac{y_x}{x_y}\left(\frac{x_y}{\gamma_a^2} - x\right)\left(\frac{x_z}{\beta_a^2} - x\right)$$

$$= \frac{\gamma_b^2}{\alpha_b^2\gamma_a^2}\cdot\frac{y_z x_y}{y_x}x + \left(\frac{x_y}{\gamma_a^2} - x\right)\left(\frac{x_z}{\beta_a^2} - x\right); \text{ or,}$$

$$0 = \frac{\beta_a^2 - \gamma_a^2}{\beta_a^2\gamma_a^2}(x_y - x_z)x + \left(\frac{x_y}{\gamma_a^2} - x\right)\left(\frac{x_z}{\beta_a^2} - x\right) \text{by } (17_1) \text{ and } (17_1^1)$$

$$= x^2 - \left(\frac{x_y}{\beta_a^2} + \frac{x_z}{\gamma_a^2}\right)x + \frac{x_y}{\beta_b^2}\cdot\frac{x_z}{\gamma_a^2} \therefore x = \frac{x_z}{\gamma_a^2} \text{ or } x = \frac{x_y}{\beta_a^2}$$

Corresponding to the first value we have:

$$y = \frac{\gamma_a^2}{\gamma_b^2}\cdot\frac{y_x}{x_y}\cdot\frac{x_y - x_z}{\gamma_a^2} = \frac{y_x}{\gamma_b^2}\cdot\frac{x_y - x_z}{x_y} = \frac{y_z}{\gamma_b^2} \text{ by } (17_1)$$

and corresponding to the second:

$$y = \frac{\gamma_a^2}{\gamma_b^2}\cdot\frac{y_x}{x_y}\cdot x_y\left(\frac{1}{\gamma_a^2} - \frac{1}{\beta_a^2}\right) = \frac{y_x}{\gamma_b^2}\cdot\frac{\beta_a^2 - \gamma_a^2}{\beta_a^2} = \frac{y_x}{\alpha_b^2} \text{ by } (17_1^1)$$

Denoting these constants by x_o, x_b, y_o, y_a, respectively, we have then the equations of a pair of generatrices of the hyperboloid (13) perpendicular to the xy-plane:

$$x = \frac{x_z}{\gamma_a^2} = x_o\ ;\ y = \frac{y_z}{\gamma_b^2} = y_o \qquad (20_z)$$

$$x = \frac{x_y}{\beta_a^2} = x_b\ ;\ y = \frac{y_x}{\alpha_b^2} = y_a \qquad (20_z^1)$$

Similarly the pair of generatrices perpendicular to the yz-plane:

$$y = \frac{y_x}{\alpha_b^2} = y_a\ ;\ z = \frac{z_x}{\alpha_o^2} = z_a \qquad (20_x)$$

$$y = \frac{y_z}{\gamma_b^2} = y_o\ ;\ z = \frac{z_y}{\beta_o^2} = z_b \qquad (20_x^1)$$

and that perpendicular to the zx-plane:

$$z = \frac{z_y}{\beta_o^2} = z_b\ ;\ x = \frac{x_y}{\beta_a^2} = x_b \qquad (20_y)$$

$$z = \frac{z_x}{\alpha_o^2} = z_a\ ;\ x = \frac{x_z}{\gamma_a^2} = x_o \qquad (20_y^1)$$

Now the second line of each pair intersects the chord, as may be proved thus: The equations of the chord (1, 2) are any two of the following three equations:

$$\frac{x}{x_y} + \frac{y}{y_x} - 1 = 0 \qquad (21_z)$$

$$\frac{y}{y_z} + \frac{z}{z_y} - 1 = 0 \qquad (21_x)$$

$$\frac{z}{z_x} + \frac{x}{x_z} - 1 = 0 \qquad (21_y)$$

Now $\frac{x_b}{x_y} + \frac{y_a}{y_x} - 1 = \frac{1}{\beta_a^2} + \frac{1}{\alpha_b^2} - 1 = \alpha_b^2 + \beta_a^2 - \alpha_b^2 \beta_a^2 = 0$

and (21_x) or (21_y) can always be satisfied for some value of z; therefore (20_z^1) intersects the chord. In the same manner it may be proved that (20_x^1) and (20_y^1) intersect the chord. It follows, then, that (20_z), (20_x), and (20_y) cannot intersect the chord, and hence belong to the same system of generation.

The equations of a pair of lines intersecting in a given point of the hyperboloid and belonging to different systems of generation can be easily found by the condition that one of them must intersect (20) and the other (20^1). I omit this, but give a remarkable symmetrical form of the equation of the hyperboloid:

$$0 = (x - x_b)(y - y_o)(z - z_a) - (x - x_o)(y - y_a)(z - z_b) \qquad (22)$$

$$= x(y_o z_a - y_a z_b) + y(z_a x_b - z_b x_o) + z(x_b y_o - x_o y_a)$$

$$- xy(z_a - z_b) - yz(x_b - x_o) - zx(y_o - y_a), \text{ because } x_b y_o z_a = x_o y_a z_b$$

by (18) and (18^1).

It is immediately evident that this equation is satisfied by equations (20). It is not uninteresting to prove that it also satisfies (21), or that it contains the chord, since it shows the remarkable pliability of these forms by virtue of the relations (16), (17), (18), (16^1), (17^1), (18^1).

The points (x_o, y_o, z_a), (x_o, y_o, z_b), (x_b, y_o, z_b), (x_b, y_a, z_b), (x_b, y_a, z_a), (x_o, y_a, z_a) form a warped hexagon, which lies wholly in the hyperboloid, and its sides may be considered six intersecting edges of a characteristic parallelopipedon. These edges are:

$$A = \frac{1}{2}(x_b - x_o); \quad B = \frac{1}{2}(y_o - y_a); \quad C = \frac{1}{2}(z_a - z_b) \qquad (23)$$

and the co-ordinates of its center are:

$$x_o = \frac{1}{2}(x_b + x_o); \quad y_o = \frac{1}{2}(y_o + y_a); \quad z_o = \frac{1}{2}(z_a + z_b) \qquad (24)$$

and these must be those of the center of the hyperboloid also.

Transferring the origin of co-ordinates to this center, we have the equation of the hyperboloid regarding (23):

$$0 = (x - A)(y - B)(z - C) - (x + A)(y + B)(z + C) \qquad (25)$$

From this equation we soon find by familiar processes the lengths and directions of the principal axes.

As to the question, Which of the alignment curves should be used in geodesy? I observe that between two intervisible points on the terrestrial spheroid the difference between the course of these curves is so extremely minute that they are practically identical; we can use then that method of tracing which is most convenient. For the distance of non-intervisible stations I consider the brachisthode the *geodetic* line as heretofore, because 1st, the diorthode becomes impracticable; and 2d, it cannot be divided into portions which are themselves diorthodes. As Assistant Wm. Eimbeck, of the United States Coast and Geodetic Survey, suggested to me, the diorthode proper cannot even be traced between very distant stations, which are intervisible only from very elevated positions, such as high peaks or the usual wooden structures. This led me to consider a new class of alignment curves—the apparent horizon alignment curves. The *a. h.* pro-orthode would be the locus of all points for which the tangent cuts the normal at the forward end; while the *a. h.* diorthode is a curve, at any point of which a tangent to the surface, which passes through the normal at one end, also passes through that at the other end. The equation (3) being adapted to these changed conditions will furnish also the equations of these curves; and I have thus found that the *a. h.* diorthode on an ellipsoid has an intersecting surface of the fourth order.

Messrs. HARKNESS and DOOLITTLE made remarks on this paper.

Mr. ASAPH HALL then made a communication on

THE DETERMINATION OF THE MASS OF A PLANET FROM OBSERVATIONS OF TWO SATELLITES.

[Abstract.]

M. Struve recommends that the position angle and distance of one satellite from another satellite be measured, instead of referring the place of each to the center of the primary planet; and a series of such measurements on satellites of Jupiter has been begun under his direction at Pulkowa. These observations are found to occupy one-third the time, and are considered two or three times as accurate as those where the planet is used. The most important advantage of the new method is its freedom from the unknown constant errors attending the old, due to the great difference in size and bright-

ness of the objects measured. The price to be paid for this advantage is a greatly increased complexity in the computation; for the elements of both orbits now enter into each equation of condition, and there are therefore twelve normal equations instead of six to solve. The comparative difficulty may be estimated by the number of auxiliary quantities that must be computed in the solution of n equations, namely:

$$\frac{1}{6} n (n + 1) (n + 5),$$

which amounts to 77 for $n = 6$, and to 442 for $n = 12$; a value nearly six times as great. But it is worth while to bear in mind that the twelve equations, by giving the elements and mean distance of each satellite, give two values of the planet's mass.

Mr. HARKNESS called attention to the advantage of substituting an accidental error, be it even a large one, for an unknown constant error.

Mr. TAYLOR criticised the designations usually given to the apsides of satellites orbits as being particular when they should be general. He suggested the terms *peri-apsis* and *apo-apsis*, or *aphapsis*.

Remarks were also made by Messrs. KUMMELL and HILL.

Before adjournment the Chairman replied to some questions as to the new object glass for the Imperial Observatory at Pulkowa; and gave a short explanation of the difficulty of calculating the true anomaly in elliptic orbits.

4TH MEETING. MAY 9, 1883.

The Chairman presided.

Present: twelve members and one guest.

The report of a committee appointed by the General Committee of the Society to consider matters pertaining to Sections was read.

Mr. DOOLITTLE read a paper entitled

INFINITE AND INFINITESIMAL QUANTITIES.

[Abstract.]

An infinitesimal may be defined as the result of infinite division;

but the term *infinite division* probably does not represent the same conception to all mathematicians. If we suppose a quantity divided into a number of parts, and each of these parts subdivided, and similar subdivisions to go on forever, each requiring finite time, we have a conception to which the name *infinite division* may be given with some appropriateness, but which might better be called *eternal division*. Such division never reaches a result. But if we suppose the time of each subdivision to be proportional to the magnitude of each part, the entire process is completed in finite time, although no limit can be given to the number of subdivisions. If a point be supposed to have passed with constant velocity over a given distance, there was a time when it had passed over half the distance; afterward a time when the remaining distance was one-fourth of the original distance; the number of such successive halvings is certainly unlimited; and the result is that there is no remaining distance. This is division infinite but not eternal, and the result seems to be zero.

As a point is defined to be position without magnitude, so may an infinitesimal be defined to be quantitative relation without magnitude. The terms *infinitesimal, differential, nothing,* and *zero,* are not synonyms. They have the same logical denotation but differ in connotation. Mathematicians usually speak of "the value" or "the true value" of a vanishing fraction, as though any quantity whatever were not a true value. The term *serial value* is proposed as conducive to clearness of thought. A differential coefficient is the serial value of a vanishing fraction; and a differential or infinitesimal may be further defined as zero in serial relation to continuously diminishing quantity.

The term *infinitesimal* is however frequently employed like other terms to denote the symbol of its exact signification. We speak of drawing and erasing lines, meaning the visible symbols of Euclidean lines. Even in our purely mental processes we give the name *points* to the imagined small volumes that symbolize positions without magnitude. In like manner the term *infinitesimal* is employed to denote the imagined small quantity in approximate relation that symbolizes a relation which becomes exact only when magnitude disappears.

A line is infinite relatively to a point, but infinitesimal, *i. e.*, zero, relatively to a surface or volume. Every quantity is finite relatively to other quantities of its own order—zero relatively to orders above and infinite relatively to orders below. A volume is integrated from surfaces, a surface from lines, and a line from points. Each integral is infinite relatively to the magnitudes from which it is integrated. As momentum is integrated from motion-generating force, it is infinite relatively thereto. Momentum may also be dissipated by infinitesimal decrements; and it is possible that momentum is always thus dissipated and re-integrated whenever motion is communicated from one body to another; but the principles of mathematics are equally consistent with the hypothesis that actual contact sometimes occurs, in which case motion is directly and instantaneously transmitted without dissipation or re-integration. Granting that infinitesimal time requires infinite force, momentum satisfies that condition.

This paper gave rise to considerable discussion, in which Messrs. TAYLOR, HILL, KUMMELL, and LEFAVOUR maintained the legitimacy of the notion of infinitesimals as real elements out of which quantity is built up; Messrs. ELLIOTT, DOOLITTLE, and FARQUHAR took the opposite ground, preferring the Newtonian view of the Calculus; while Mr. CHRISTIE, while preferring the infinitesimal method, maintained that no evaluation of continuous quantity, in terms of units as it must necessarily be, could ever be precise or entirely satisfactory, to however small a compass the uncertainty be reduced. Mr. CHRISTIE also pointed out some paradoxes to which the usual definitions of curves and tangents appeared to lead.

Mr. ELLIOTT then exhibited some tables to serve as a perpetual calendar, and gave a full explanation how by means of them the day of the week corresponding to that of the month for any year, New or Old Style, B. C. or A. D., could be found.

5TH MEETING. MAY 23, 1883.

The Chairman presided.

Twenty members and guests present.

The appointment of the committee called for under the new Standing Rule relating to papers read before Sections of the Society was considered. Mr. TAYLOR moved that the committee consist of the Chairman and Secretary and a third member to be appointed by the Chair. After some discussion by Messrs. HARKNESS and ELLIOTT it was so ordered, with the additional provision that this appointment be made for each paper separately.

Mr. G. W. HILL made a communication on

PLANETARY PERTURBATIONS OF THE MOON,

which was yet unfinished when he yielded the floor to Mr. G. K. GILBERT, who made a communication on

GRAPHIC TABLES FOR COMPUTING ALTITUDES FROM BAROMETRIC DATA.

This paper will appear in the Bulletins of the U. S. Geological Survey.

6TH MEETING. JUNE 6, 1883.

The Chairman presided.

Present, sixteen members and guests.

Mr. G. W. HILL concluded his paper on

CERTAIN POSSIBLE ABBREVIATIONS IN THE COMPUTATION OF THE LONG-PERIOD PERTURBATIONS OF THE MOON'S MOTION DUE TO THE DIRECT ACTION OF THE PLANETS.

[Abstract.]

Hansen has characterized the calculation of these inequalities as extremely difficult. However, it seems to me that if the shortest methods are followed there is no ground for such an assertion. The work may be divided into two portions independent of each other. In one the object is to develop, in periodic series, certain functions of the moon's coördinates, which in number do not exceed five. This portion is the same whatever planet may be considered to act, and hence may be done once for all. In the other portion we seek the coefficients of certain terms in the periodic development of certain functions, five also in number, which involve the coördinates of the earth and planet only. And this part of the work is very similar to that in which the perturbations of the earth by the planet in question are the things sought. And as the multiples of the mean motions of these two bodies, which enter into the expres-

sion of the argument of the inequalities under consideration, are necessarily quite large, approximate values of the coefficients may be obtained by semi-convergent series similar to the well-known theorem of Stirling. This matter was first elaborated by Cauchy,* but in the method as left by him we are directed to compute special values of the successive derivatives of the functions to be developed. Now it unfortunately happens that these functions are enormously complicated by successive differentiation, so that it is almost impossible to write at length their second derivatives. Manifestly then, it would be a great saving of labor to substitute for the computation of special values of these derivatives a computation of a certain number of special values of the original function, distributed in such a way that the maximum advantage may be obtained. This modification has given rise to an elegant piece of analysis.

It will be noticed that in this method it is necessary to substitute in the formulæ, from the outset, the numerical values of the elements of the orbits of the earth and planet. There seems to be no objection to this on the practical side, as for the computation of the inequalities sought no partial derivatives of R, with respect to these elements, are required.

The paper is printed in full in the *American Journal of Mathematics*, Vol. VI.

Mr. E. B. Elliott made a communication on

UNITS OF FORCE AND ENERGY, INCLUDING ELECTRIC UNITS.

SEVENTH MEETING. NOVEMBER 21, 1883.

The Chairman presided.

Thirteen members present.

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* Mémoire sur les approximations des fonctions de très-grands nombres, and Rapport sur un Mémoire de M. Le Verrier, qui a pour objet la détermination d'une grande inégalité du moyen mouvement de la planète Pallas. Comptes Rendus de l'Académie des Sciences de Paris. Tom. XX, pp. 691–726, 767–786, 825–847.

Mr. C. H. KUMMELL read a communication entitled

THE THEORY OF ERRORS PRACTICALLY TESTED BY TARGET-SHOOTING.

[Abstract.]

Sir John Herschel treats a special case in which shots of equal probability are in circles. According to Liagre's theory target shooting is compounded of two distinct operations, viz., sighting and leveling, each of which is liable to errors, independently following the ordinary linear law of error. Some reasons for the independence of these operations are that for sighting the direction of the wind, which does not affect the leveling, must be regarded; and that, on the other hand, leveling only is affected by the range. The consequences of Liagre's theory will now be developed.

Let x = error of sighting and ε_x its mean error;

y = error of leveling and ε_y its mean error;

then it follows that

$\frac{dx}{\varepsilon_x\sqrt{2\pi}} e^{-\frac{x^2}{2\varepsilon_x^2}}$ = probability to hit anywhere at distance x from sighting axis. (1_x)

$\frac{dy}{\varepsilon_y\sqrt{2\pi}} e^{-\frac{y^2}{2\varepsilon_y^2}}$ = probability to hit anywhere at distance y from leveling axis. (1_y)

$\therefore \frac{dxdy}{2\varepsilon_x\varepsilon_y\pi} e^{-\frac{x^2}{2\varepsilon_x^2}-\frac{y^2}{2\varepsilon_y^2}}$ = probability to hit the point (x, y). (2)

This probability is the same for any point on the ellipse:

$$\frac{x^2}{\varepsilon_x^2} + \frac{y^2}{\varepsilon_y^2} = \frac{r^2}{\varepsilon^2}, \text{ where } \varepsilon^2 = \frac{1}{2}(\varepsilon_x^2 + \varepsilon_y^2) \quad (3)$$

This I shall call, then, an equal probability ellipse; its semi-axes are:

$$\frac{\varepsilon_x}{\varepsilon} r \text{ and } \frac{\varepsilon_y}{\varepsilon} r \quad (4)$$

and r = mean semi-diameter (which is equal to its conjugate).

Assume $\quad x_r = \frac{\varepsilon}{\varepsilon_x} \quad$ and $\quad y_r = \frac{\varepsilon}{\varepsilon_y} y \quad$ (5)

then every point on the equal probability ellipse (3) corresponds to a point (x_r, y_r) on the circle: $x_r^2 + y_r^2 = r^2$, (6)
which is the reduced equal probability circle.

Counting directions from the right of the x - axis, let

$$a = \text{direction of } (x, y) \tag{7}$$

$$a_r = \text{ " " } (x_r, y_r), \text{ or reduced direction of } (x, y) \tag{8}$$

then $\tan a = \frac{y}{x} = \frac{\varepsilon_y}{\varepsilon} y_r \div \frac{\varepsilon_x}{\varepsilon} x_r = \frac{\varepsilon_y}{\varepsilon_x} \tan a_r$ (9)

also $x = \frac{\varepsilon_x}{\varepsilon} r \cos a_r$ (10_x)

$y = \frac{\varepsilon_y}{\varepsilon} r \sin a_r$ (10_y)

whence $dx = \frac{\varepsilon_x}{\varepsilon} \cos a_r \, dr - \frac{\varepsilon_x}{\varepsilon} r \sin a_r da_r$

$dy = \frac{\varepsilon_y}{\varepsilon} \sin a_r dr + \frac{\varepsilon_y}{\varepsilon} r \cos a_r da_r$

Transforming, then, (2) to the new variables, r and a_r, we must replace:

$$dx\,dy \text{ by } \frac{\varepsilon_x \varepsilon_y r dr da_r}{\varepsilon^2}$$

and thus obtain

$\frac{r dr da_r}{2\pi\varepsilon^2} e^{-\frac{r^2}{2\varepsilon^2}}$ = probability to hit a point of which (r, a_r) is the reduced point. (11)

Fig. 1

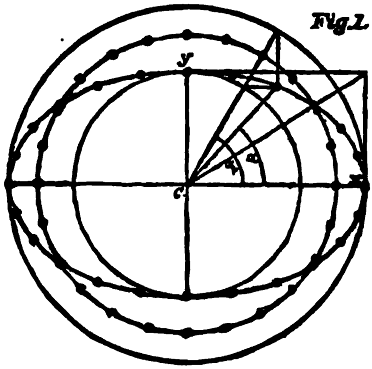


Fig. 1 exhibits 24 shots of equal probability, on an equal probability ellipse, and their reduced positions evenly distributed over the reduced circle.

The probability to hit anywhere on the perimeter of an equal probability ellipse of mean semi-diameter, r, is found by integrating (11), with respect to a_r, through a circumference. It is

$$P_r = \frac{rdr}{\varepsilon^2} e^{-\frac{r^2}{2\varepsilon^2}} \tag{12}$$

Let n_r = number of shots on area of equal probability ellipse of semi-diameter r, and n = total number; then

$$\frac{n_r}{n} = \int_0^r \frac{rdr}{\varepsilon^2} e^{-\frac{r^2}{2\varepsilon^2}} = 1 - e^{-\frac{r^2}{2\varepsilon^2}} \text{ or } \frac{n - n_r}{n} = e^{-\frac{r^2}{2\varepsilon^2}} \tag{13}$$

Let $r = \rho$; if $n_r = \frac{1}{2}n$, then $\frac{1}{2} = e^{-\frac{\rho^2}{2\varepsilon^2}} \therefore \rho = \varepsilon \sqrt{2l2}$ (14)

The ellipse:

$$\frac{x^2}{\varepsilon_x^2} + \frac{y^2}{\varepsilon_y^2} = 2l2 \tag{15}$$

is then an even chance ellipse, which is hit or missed with equal probability. Eliminating ε between (13) and 14), we obtain:

$$\left(\frac{n - n_r}{n}\right)^{\rho^2} = \left(\frac{1}{2}\right)^{r^2} \tag{16}$$

$$\therefore \rho = r \sqrt{\frac{\log 2}{\log \left(\frac{n}{n - n_r}\right)}} \tag{17}$$

These formulæ agree with Herschel's in form, and have, also, the same signification, in case the precisions of sighting and leveling are equal, for in that case the ellipses (3) and (15) become circles and r, ρ their radii, respectively. Herschel employs these formulæ for determining the skill of a marksman, which he defines to be $= \frac{1}{\rho}$, from the number of shots that have fallen on a circle of radius r.

Correspondingly, we should have to count the shots that have fallen on an equal probability ellipse, the axes of which have the unknown ratio $\frac{\varepsilon_y}{\varepsilon_x}$, which, as yet, we have no method of finding; therefore formulæ (14) and (17) cannot be employed in their general signification. If, nevertheless, we count the shots on a circle of radius r and compute a value for ρ and ε, we shall come as near to their true values as the problem requires, especially if the precisions of sighting and leveling are not very different. This can be shown analytically by proving that the probability of hitting the area of the circle

$$x^2 + y^2 = r^2$$

differs from that of hitting the equal probability ellipse

$$\frac{x^2}{\varepsilon_x^2} + \frac{y^2}{\varepsilon_y^2} = \frac{r^2}{\varepsilon^2}$$

by terms of the fourth order, with respect to the difference between the mean errors of sighting and leveling.

In computing ρ by (17) the radius (or mean semi-diameter) r is left arbitrary; it is, however, not at all indifferent; for if we take it very small or very large it will give very unreliable values of ρ. There must then be a certain magnitude of r giving the most reliable value of ρ, and it is that which makes $P_r = \frac{rdr}{\varepsilon^2} e^{-\frac{r^2}{2\varepsilon^2}}$ a maximum. This gives the condition: $0 = \frac{1}{\varepsilon^2} - \frac{r^2}{\varepsilon^4} \therefore r = \varepsilon$

Thus the most favorable value of r for determining ρ is the mean error ε and the ellipse $\frac{x^2}{\varepsilon_x^2} + \frac{y^2}{\varepsilon_y^2} = 1$ (18)

is the ellipse of the most probable shot.

Placing $r = \varepsilon$ in (13), we have

$$\frac{n - n_\varepsilon}{n} = e^{-\frac{1}{2}} = 0.60653 \ldots .$$

$$\therefore\ n_\varepsilon = \left(1 - e^{-\frac{1}{2}}\right) n = 0.39347 \ldots n = 0.4n \text{ nearly} \qquad (19)$$

The most probable shot is, therefore, the distance of the $(0.4n)$th shot from the center nearly; also the mean of the $(0.4n + m)$th, and the $(0.4n - m)$th shot should, if m is not too large, give a fair value of the most probable shot.

Solving (13) for ε, we have also

$$\varepsilon = \frac{r}{\sqrt{2l \frac{n}{n - n_r}}} \qquad (20)$$

From the definition of ε_x and ε_y it is obvious that

$$\varepsilon_x = \sqrt{\frac{[x^2]}{n}};\ \varepsilon_y = \sqrt{\frac{[y^2]}{n}} \qquad (21)$$

which formulæ afford a comparison between the precisions of sighting and leveling. We have then

$$\varepsilon = \sqrt{\frac{[s^2]}{2n}} \text{ if } s = \sqrt{x^2 + y^2} \tag{22}$$

This formula, although laborious for practical use, is the most rigorous measure of skill in shooting, and there is no need of other formulæ except when shots are lost. In that case it requires an important modification, whereby it loses in rigor if the number of lost shots is considerable. Assuming the precisions of sighting and leveling equal, then the reduced distance r in (12) will be the actual distance s of a shot; and if the target is circular, of limiting radius R, we have

$$[s^2]_1^{n_R} = n \int_0^R s^2 \frac{s ds}{\varepsilon^2} e^{-\frac{s^2}{2\varepsilon^2}}$$

$$= n \left\{ -R^2 e^{-\frac{R^2}{2\varepsilon^2}} + 2\varepsilon^2 \left(1 - e^{-\frac{R^2}{2\varepsilon^2}}\right) \right\}$$

Now by (13) $$n_R = n\left(1 - e^{-\frac{R^2}{2\varepsilon^2}}\right)$$

therefore $$[s^2]_1^{n_R} = 2n_R \varepsilon^2 - \left(n - n_R\right) R^2$$

and $$\varepsilon^2 = \frac{[s^2]_1^{n_R} + \left(n - n_R\right) R^2}{2n_R} \tag{23}$$

This formula reverts, of course, to (22), if $n = n_R$, and it makes the most probable sum of the squares of the lost shots

$$[s^2]_{n_R}^{n} = \frac{n}{n_R}\left(n - n_R\right) R^2$$

and since $\left(n - n_R\right) R^2$ is the smallest possible actual value of this quantity; this expression for it is quite plausible.

The targets used by the National Rifle Association are rectangular. (At long range they are 12 feet wide and 6 feet high).

Let a (= 6 feet) be the limiting value of x and b (= 3 feet) that for y, then we have, if n_{ab} is the number of hitting shots

$$n_{ab} = n \int_{-a}^{a} \frac{dx}{\varepsilon_x\sqrt{2\pi}} e^{-\frac{x^2}{2\varepsilon_x^2}} \int_{-b}^{b} \frac{dy}{\varepsilon_y\sqrt{2\pi}} e^{-\frac{y^2}{2\varepsilon_y^2}} = n\, Pt_a Pt_b \qquad (24)$$

The integral $Pt_a = \int_{-a}^{a} \frac{dx}{\varepsilon_x\sqrt{2\pi}} e^{-\frac{x^2}{2\varepsilon_x^2}} = \frac{2}{\sqrt{\pi}} \int_0^{t_a} dt\, e^{-t^2}$, and

similarly Pt_b, is tabulated in Chauvenet's Method of Least Squares (Table IX, appendix, to the argument t), and is therefore known.

We have further:

$$[x^2]_I^{n_{ab}} = n \int_{-a}^{a} x^2 \frac{dx}{\varepsilon_x\sqrt{2\pi}} e^{-\frac{x^2}{2\varepsilon_x^2}} \int_{-b}^{b} \frac{dy}{\varepsilon_y\sqrt{2\pi}} e^{-\frac{y^2}{2\varepsilon_y^2}}$$

$$= nPt_b \left\{ -\left[\frac{\varepsilon_x x}{\sqrt{2\pi}} e^{-\frac{x^2}{2\varepsilon_x^2}}\right]_{-a}^{a} + \varepsilon_x^2 \int_{-a}^{a} \frac{dx}{\varepsilon_x\sqrt{2\pi}} e^{-\frac{x^2}{2\varepsilon_x^2}} \right\}$$

$$= n\varepsilon_x^2 Pt_b\,(Pt_a - t_a P't_a) \qquad (25_x)$$

Here $P't_a$ denotes $\frac{dPt_a}{dt_a} = \frac{2}{\sqrt{\pi}} e^{-t_a^2}$ and can also be taken from Chauvenet's table, being 100 × difference. Similarly,

$$[y^2]_I^{n_{ab}} = n\varepsilon_y^2 Pt_a\,(Pt_b - t_b P't_b) \qquad (25_y)$$

By virtue of (24) we have also

$$\varepsilon_x^2 = \frac{[x^2]_I^{n_{ab}}}{n_{ab}\left(1 - t_a \frac{P't_a}{Pt_a}\right)} \qquad (25_x')$$

$$\varepsilon_y^2 = \frac{[y^2]_I^{n_{ab}}}{n_{ab}\left(1 - t_b \frac{P't_b}{Pt_b}\right)} \qquad (25_y')$$

and these formulæ may be used to compute ε_x and ε_y by an obvious approximative process. They show that $\varepsilon_x^2 > \frac{[x^2]_I^{n_{ab}}}{n_{ab}}$, as it should be; but it may, or rather must, happen sometimes that the most probable increase of the sum of x^2 and y^2 or $[x^2]_{n_{ab}}^{n} + [y^2]_{n_{ab}}^{n}$ consistent with (25′) is $< (n - n_{ab})\, b^2$, b being the smaller limit. Such a result cannot be accepted, being contradictory to the fact that there are $n - n_{ab}$ shots at a greater distance than b. The following method gives plausible results in that case. Assume

$$(\epsilon_y^2) = \frac{[y^2]_1^{n_{ab}} + (n - n_{ab})b^2}{n} \qquad (b<a) \qquad (25_y'')$$

as first approximate value in $(25_y')$, and if $\epsilon_y < (\epsilon_y)$ adopt (ϵ_y) as final value of ϵ_y: but if $\epsilon_y > (\epsilon_y)$, then proceed in approximating to ϵ_y by $(25_y')$. The solution of $(25_x')$ gives, as heretofore, the best value of ϵ_x. Among the target records of the international shooting match of 1874, at Creedmoor, there are 9 with lost shots, 5 of which give too small an increase of sum of squares, and this means that from the record of the hitting shots it would not appear probable that so many shots were lost.

Instead of the squares, we may, however, employ first powers of distances; and I shall develop the requisite formulæ for a circular target and equal precisions.

$$\text{We have } [s]_1^{n_R} = n\int_0^R s\,\frac{s\,ds}{\epsilon^2}\,e^{-\frac{s^2}{2\epsilon^2}}$$

$$= n\left(-Re^{-\frac{R^2}{2\epsilon^2}} + \epsilon\sqrt{\frac{\pi}{2}}\,Pt_R\right)$$

$$= -\left(n - n_R\right)R + n\epsilon\sqrt{\frac{\pi}{2}}\,Pt_R \qquad \text{by (13)}$$

$$\therefore \epsilon = \frac{[s]_1^{n_R} + \left(n - n_R\right)R}{nPt_R}\sqrt{\frac{2}{\pi}} \qquad (26)$$

If $n_R = n$, this becomes $\epsilon = \frac{[s]}{n}\sqrt{\frac{2}{\pi}}$ (27)

The quantity $r_0 = \frac{[s]}{n} = \epsilon\sqrt{\frac{\pi}{2}}$ (28)

which may be called the *average shot*, has been recently introduced by the United States Ordnance Department, under the name "radius of the circle of shots," in place of the extremely defective quantity, the mean absolute deviation, the insufficiency of which was pointed out by Henry Metcalfe, Captain of Ordnance, in the Report of the Chief of Ordnance of 1882. Thus the adopted method of discussion of the precision of firearms, as used by that department, is in agreement with Liagre's theory, only the shots are not referred to the true center, but to the "center of shots," viz.: their center of gravity.

We have, now, the following three quantities, each of which may be used as a measure of precision, sighting and leveling being equally good..

1, the even chance shot, ρ.

2, the most probable shot, ϵ, (or mean error of sighting and leveling).

3, the average shot, r_0, also called radius of the circle of shots; and they are related to each other as follows:

$$\frac{\rho}{\sqrt{2 l 2}} = \epsilon = r_0\sqrt{\frac{2}{\pi}} \tag{29}$$

The preceding formulæ I regard as complete, for practical discussion of target records, provided there is no evidence for a constant vitiating cause. If, for example, during a shooting match the wind is blowing constantly in the same direction, the effect of this might be partially revealed by computing for the whole match the quantity:

$$x_0 = \frac{[x]}{n} \tag{30}$$

If the sign of this quantity is consistent with the observed direction of the wind, it might, perhaps, be proper to refer the shots to a new center, to the right or left of the true center, by this quantity. In that case we have, however,

$$\epsilon_x = \sqrt{\frac{[x^2] - nx_0^2}{n-1}} \tag{31}$$

In leveling there may be a somewhat constant individual habit of holding too high or too low, which, however, ought not to be eliminated in a fair discussion of a match, although it would be of interest to compute the quantity

$$y_0 = \frac{[y]}{n}$$

for each marksman and for a whole team.

Much less proper, it would seem to me, to regard the position of the axes unknown, and to compute their most probable position. If center and axes are to be determined, x' y' denote the co-ordinates of a shot from a random origin and position of axes, and w the angle of turning the latter into their most probable direction; then the most probable co-ordinates of a shot are:

$$x = x_0 + x' \cos w + y' \sin w; \quad y = y_0 + y' \cos w - x' \sin w.$$

Imposing the conditions of a minimum for $[x^2]$ and $[y^2]$, we find

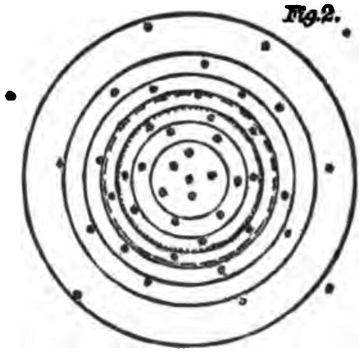
$$\left.\begin{aligned} x_0 &= -\frac{1}{n}\Big([x'] \cos w + [y'] \sin w\Big); \\ y_0 &= -\frac{1}{n}\Big([y'] \cos w - [x'] \sin w\Big) \end{aligned}\right\} \quad (33)$$

$$\tan 2w = \frac{[x'y'] - \frac{1}{n}[x'][y']}{[x'^2] - \frac{1}{n}[x']^2 - [y'^2] + \frac{1}{n}[y']^2} \quad (34)$$

These formulæ have, however, their proper place in the theory of Andræ's "Fehler-ellipse."

Fig. 2 exhibits an ideal distribution of 45 shots. Each ring contains 6 shots, leaving 3 shots between the outer ring and infinity. The dotted circle is that of the most probable shot, and the dashed one that of the even chance shot.

Fig. 2.



The following table refers to the combined target record of the Irish team at 800 yards range, in the international shooting match of 1874, at Creedmoor:

Irish Team at 800 *yards:* $\epsilon = 1.3095 ft.$; 90 *shots,* 88 *hits.*

	Radii.	No. of shots on circle.		Discrepancy.	No. of shots on ring.		Discrepancy.
		Theory.	Actual.		Theory.	Actual.	
	Feet.						
	0.5	6 3	5	+1.3	6.3	5	+1.3
	1.0	22.8	22	+0.8	16.5	17	—0.5
	1.5	43.3	47	—3.7	20.5	25	—4.5
	2.0	62.0	58	+4.0	18.7	11	+7.7
	2.5	75.5	74	+1.5	13.5	16	—2.5
Leveling limit..	3.0	83.5	83	+0 5	8.0	9	—1.0
	3 5	87.5	87+ ?	+0.5?	4.0	4+ ?	0.0?
	4.0	89.2	87+ ?	?	1.7	?	?
	4.5	89.8	88+ ?	?	0.6	1+ ?	—0.4?
					89.8	88+2	

A target of 50 pistol shots at 50 yards range shows similar discordance between theory and practice, which, on an average, may be taken less than 5 per cent.

Target of pistol shots at 50 yards range: $\epsilon = 0.167$ *ft.; 50 shots, no misses.*

Radii.	No. of shots on circle.		Discrep'y.	No. of shots on ring.		Discrep'y.
	Theory.	Actual.		Theory.	Actual.	
in.						
0.5	1.5	1	+0.5	1.5	1	+0.5
1.0	5.9	8	—2.1	4.4	7	—2.6
1.5	12.2	14	—1.8	6.3	6	+0.3
2.0	19.7	23	—3.3	7.5	9	—1.5
2.5	27.0	28	—1.0	7.3	5	+2.3
3.0	33.7	33	+0.7	6.7	5	+1.7
3.5	39.6	37	+2.6	5.9	4	+1.9
4.0	43.2	41	+2.2	3.6	4	—0.4
4.5	46.0	46	0.0	2.8	5	—2.2
5.0	47.8	47	+0.8	1.8	1	+0.8
5.5	48.8	49	—0.2	1.0	2	—1.0
6.0	49.4	50	—0.6	0.6	1	—0.4
				49.4	50	

Mr. ELLIOTT gave an example of remarkably close agreement between the distribution of errors by theory and by observation of the chest measurements of 1,516 United States soldiers, reported by Dr. Bulkley at the Berlin Statistical Congress. In five groups the greatest difference was four-tenths per cent.

EIGHTH MEETING. DECEMBER 5, 1883.

The Chairman presided.

Fourteen members and guests present.

Mr. ALVORD discussed

A SPECIAL CASE IN MAXIMA AND MINIMA,

the problem being to find the radius of the sphere that will displace the maximum quantity of liquid from a conical wine glass full of water.

The differential co-efficient, when put equal to zero, is in the form of two factors. Equating each to zero, one gives the radius of the maximum sought; the other gives a still larger radius, which proves to be the radius of the sphere just tangent to the centre of the base of the cone, and to the sides of the cone, extended upwards. This gives the minimum displacement equal to zero. Calling a the radius of the base, b the height, and c the slant height of the cone, theradius of the sphere producing maximum displacement equals $\frac{abc}{(c-a)(2a+c)}$; the radius corresponding to minimum displacement equals $\frac{ab}{c-a}$.

When the radius is still greater, the sphere does not reach the surface of the liquid, but displaces an imaginary quantity of the same. An analytical expression for this case was sought in vain; the result above is simple, and no square root of a negative quantity appears. By some device in the mode of investigation, this imaginary case might appear, as in the question to obtain the radical axis of two circles, discussed by Salmon.

Mr. KUMMELL suggested that the close relation between the circle $x^2+y^2=R^2$ and the equilateral hyperbola $x^2-y^2=R^2$, each of which could be regarded as an imaginary branch of the other, might help us to understand many of such difficulties. He showed that the radical axis of two circles not intersecting was the common chord of two equilateral hyperbolas whose major axes were those diameters of the circles which lie in the same straight line.

Mr. ELLIOTT read a communication on

A FINANCIAL PROBLEM,

in which he gave formulæ for calculating the advantage of investment in United States Government bonds, at six or at four per cent., and making use of the banking privileges thus available, over investment at a higher rate without such privileges. The restrictions caused by the high premium on Government bonds, the bank tax, and the necessary specie reserve were all allowed for.

This paper was discussed by Messrs. HARKNESS, DE LAND, SMILEY, and others.

Mr. H. FARQUHAR presented the following

FORM OF LEAST-SQUARE COMPUTATION.

Suppose four unknown constants, A, B, C and D, are to be calculated from equations of condition of the form

$$aA + bB + cC + dD = y.$$

Arrange columns in order (1) a^2, (2) ab, (3) ac, (4) ad, (5) ay, (6) b^2, (7) bc, (8) bd, (9) by, (10) c^2, (11) cd, (12) cy, (13) d^2, (14) dy.

Add up first five columns and place under (2) to (5) the quotients of their sums divided by $\Sigma(1)$.

Put the product $\frac{\Sigma(2)}{\Sigma(1)}\Sigma(2)$ under (6), $\frac{\Sigma(2)}{\Sigma(1)}\Sigma(3)$ under (7), $\frac{\Sigma(2)}{\Sigma(1)}\Sigma(4)$ under (8), $\frac{\Sigma(2)}{\Sigma(1)}\Sigma(5)$ under (9), $\frac{\Sigma(3)}{\Sigma(1)}\Sigma(3)$ under (10), $\frac{\Sigma(3)}{\Sigma(1)}\Sigma(4)$ under (11), $\frac{\Sigma(3)}{\Sigma(1)}\Sigma(5)$ under (12), $\frac{\Sigma(4)}{\Sigma(1)}\Sigma(4)$ under (13), and $\frac{\Sigma(4)}{\Sigma(1)}\Sigma(5)$ under (14), *reversing the sign in every case.*

Then add up (6) to (9), placing under the sums of (7) to (9) their quotients divided by $\Sigma'(6)$.

Put the product $\frac{\Sigma'(7)}{\Sigma'(6)}\Sigma'(7)$ under (10), $\frac{\Sigma'(7)}{\Sigma'(6)}\Sigma'(8)$ under (11), $\frac{\Sigma'(7)}{\Sigma'(6)}\Sigma'(9)$ under (12, $\frac{\Sigma'(8)}{\Sigma'(6)}\Sigma'(8)$ under (13), and $\frac{\Sigma'(8)}{\Sigma'(6)}\Sigma'(9)$ under (14), *reversing each sign.*

Add (10) to (12), putting quotients $\frac{\Sigma''(11)}{\Sigma''(10)}$ and $\frac{\Sigma''(12)}{\Sigma''(10)}$ under the sums.

Put the product $\frac{\Sigma''(11)}{\Sigma''(10)}\Sigma''(11)$ under (13) and $\frac{\Sigma''(11)}{\Sigma''(10)}\Sigma''(12)$ under (14), *reversing the signs.*

Add (13) and (14); when $\frac{\Sigma'''(14)}{\Sigma'''(13)} = \mathrm{D}$.

Then, under (12), enter $\frac{\Sigma''(12)}{\Sigma''(11)} - \frac{\Sigma''(11)}{\Sigma''(10)}\mathrm{D} = \mathrm{C}$.

Next, under (9), enter $\frac{\Sigma'(9)}{\Sigma'(6)} - \frac{\Sigma'(8)}{\Sigma'(6)}\mathrm{D} - \frac{\Sigma'(7)}{\Sigma'(6)}\mathrm{C} = \mathrm{B}$.

Lastly, under (5), enter $\frac{\Sigma(5)}{\Sigma(1)} - \frac{\Sigma(4)}{\Sigma(1)}\mathrm{D} - \frac{\Sigma(3)}{\Sigma(1)}\mathrm{C} - \frac{\Sigma(2)}{\Sigma(1)}\mathrm{B} = \mathrm{A}$.

NOTES.—[1] The sign of summation is distinguished by an additional stroke for every additional quantity introduced under the column added up.

[2] These additional quantities, under the columns of squares, (6), (10), and (13), will evidently all be negative.

[3] This form may be extended to any number of unknown quantities, by insertion of *ae*, etc., between (4) and (5), *be*, etc., between (8) and (9), and so on. Modifications where there is a smaller number of unknown constants, and where one of them has the coefficient always unity, will be obvious.

[4] One of the quantities *a*, *b*, etc., will, in many computations, be zero when another one is significant, and *vice-versa*; as when one unknown quantity changes in the course of a series of observations. In this case we may save some columns by arranging our equation thus: $a_1\,\mathrm{A}_1 + a_2\,\mathrm{A}_2 + b\,\mathrm{B} +$ etc. $= y$ (where $a_1\,a_2 = 0$, always). Here two sums are found under columns (1) to (5), two quotients under (2) to (5), and two additional quantities placed under each of the other columns before they are summed up. The remainder of the work then proceeds as before, except that the *last* step will be duplicate.

[5] It will be found advisable always to make Σa, Σb, etc., as nearly zero as possible, so that the products will be smaller and there will be less danger of error.

[6] The computation is to be checked by applying A, B, etc., and finding the residuals of y. Then $\Sigma(a\,\Delta y)$, $\Sigma(a\,\Delta b)$, etc., should all be zero.

[7] Where but two unknown quantities are to be found, one of them with the constant coefficient unity (as $\mathrm{A} + b\,\mathrm{B} = y$), other methods will usually be preferable. Two of these will be given.

I. If the values of b are symmetrical, so that $b = \beta \pm b'_1, \beta \pm b'_2, \beta \pm b'_3$, etc., here all that is necessary to find B is to subtract the value of y for every $\beta - b'$ from that for $\beta + b'$, to multiply the remainders by b', to find $\Sigma\,(b'\Delta y)$ and divide it by $2\,\Sigma\,(b'^2)$, when the quotient will be B. If A should be wanted also—as is very often not the case—then Σy must also be found, and $A = \frac{\Sigma y}{n} - \beta B$, where n equals the number of equations.

II. In all cases we may obtain the required values by taking the difference of b and of y from the mean of the column, multiplying the residual by the former difference, thus forming columns of $\left(b - \frac{\Sigma b}{n}\right)^2$ and $\left(b - \frac{\Sigma b}{n}\right)\left(y - \frac{\Sigma y}{n}\right)$ adding these and dividing the second sum by the first. That is,

$$B = \frac{\Sigma\left\{\left(b - \frac{\Sigma b}{n}\right)\left(y - \frac{\Sigma y}{n}\right)\right\}}{\Sigma\left\{\left(b - \frac{\Sigma b}{n}\right)^2\right\}};\ \text{when } A = \frac{\Sigma y}{n} - \frac{\Sigma b}{n}B.$$

NINTH MEETING. DECEMBER 19, 1883.

The Chairman presided.

Sixteen members and guests present.

Mr. H. FARQUHAR furnished a

NOTE ON THE PROBLEM DISCUSSED BY MR. ALVORD,

in which he showed that the volume of a spherical segment of height h, $\pi h^2 (R - \frac{1}{3}h)$, being real for all values of h, both positive and negative, was to be interpreted for $h<0$ or $h>2R$ as the volume of the segment of the equilateral hyperboloid of two sheets whose axes equal R; this volume being taken with a negative sign. It was positive for negative values of h, since it must become zero when $h = 0$ by negative increments; hence the minimum of the function when $h = 0$ in such problems as the one discussed.

Mr. DOOLITTLE read a communication on

THE REJECTION OF DOUBTFUL OBSERVATIONS.

[Abstract.]

For the purposes of this discussion we may divide errors into two grand classes, and name them, from their consequences, *instructive errors* and *uninstructive errors.* The latter class includes blunders in recording, pointing on wrong objects, &c. The former consists of errors that indicate error in other observations.

I once tried the experiment of dropping a short straight piece of wire five hundred times upon a sheet of ruled paper and counting the number of intersections of the wire with a ruled line. When the end of the wire touched or nearly touched a line, and intersection was doubtful, I counted it as half an intersection. I recorded the number of intersections in groups of fifty trials, as follows: 23, 26, 28.5, 24, 31.5, 28, 27, 14, 25, 28.5. These numbers may be regarded as observations from which may be deduced the probable ratio of the length of the wire to the distance between two consecutive lines; and it seems impossible to account for the remarkable smallness of the eighth number by any supposition of uninstructive error. It is almost certain that a ratio deduced from it alone is largely in error; but it indicates that the other nine observations are somewhat in error, and that its error is needed to counterbalance theirs. If we retain it, and regard the mean of all as the most probable truth, we infer that this observation is 11.55 units in error. If we reject it, and take the mean of the other nine as the most probable truth, we infer that this observation is 12 5 6 units in error. It should be remembered that the rejection of an observation does not sweep from existence the fact of its occurrence; but merely increases its already large estimate of error. Because an error of 11.55 units is so large as to be very improbable, shall we therefore infer that an error of 12 5·6 units is more probable?

It seems very clear to me that the larger an instructive error is the more instructive it is, and the more important is it that the observation containing it *should not* be rejected. The mean of all the ten above-described observations being regarded as the most probable truth, any one of the other nine could be better spared than the eighth. On the other hand, the larger an uninstructive error is, the more important it is that the observation *should* be rejected. Whenever an observation is intelligently rejected, there is a comparison of two antecedent probabilities, viz.: that of the occurrence of an instructive error of the magnitude involved and that of the occurrence of an uninstructive error of the same magnitude. When an error is evidently so large that it cannot possibly belong to the instructive class, the antecedent probability of such an instructive error is 0; the antecedent probability of an uninstructive error is always greater than 0; and the observation should certainly be rejected. But since the theory of least squares allows no limit whatever to the possible magnitude of instructive errors, such rejection involves the admission that the method of least squares is not applicable to the case. When an observation involves a merely suspicious error, which is neither so large that instructiveness is impossible nor so small as to pass without question, it would seem reasonable that the observation should be weighted according to the relative magnitudes of the two antecedent probabilities which I have mentioned; but this can never be determined with any approach to mathematical precision.

In order to make this matter clear, let us suppose for example that ninety-nine observations of equal weight and known to be free from uninstructive error are separately written on as many cards; that the number 25 is arbitrarily written on a similar card; that these hundred cards are thoroughly shuffled; and that ten cards being then drawn at random, the following numbers appear on them: 15, 18, 14, 25, 17, 16, 15, 18, 16, 17. Let it be required to determine from these data, according to the theory of least squares, the probability that the number 25 on the fourth card drawn is the record of an observation. Here the antecedent probability of an uninstructive error is by hypothesis equal to 1-10.

I commence by assuming a value of the required probability, and weight the doubtful observation accordingly. I then proceed in the ordinary method and determine an approximation to the antecedent probability of the occurrence of a genuine observation giving the value 25 by integrating $\frac{1}{\sqrt{\pi}}\int e^{-t^2}\,dt$ between the limits corresponding to 24.5 and 25.5, since the observations are taken to the nearest unit. This integral is the antecedent probability of an instructive error of the given magnitude, tainted with the incorrectness of the assumption with which I began. Call this integral p. Then $\frac{p}{\frac{1}{10}+p}$ is the resulting required probability. If it agrees with my original assumption, the problem is solved. If it does not agree, I have data for a better assumption according to the well-known method of trial and error. After a few repetitions of the process, as I have found by experiment, an assumption can be made that will be verified by agreement with the result.

In practical problems the antecedent probability of blunders and other uninstructive errors is never known, and is only matter of exceedingly vague conjecture. Perhaps if a very large number of observations were examined, and the proportion of evidently uninstructive errors ascertained, a somewhat intelligent estimate might be made of the proportion of those that exist but are not evident; and data of some little value might be gathered toward a scientific method of weighting. But I have no faith that the result would be any where near worth the labor. At present, the best that a computer can do is to reject entirely, or retain entirely, or assign a simple weight, such as $\frac{1}{2}$, $\frac{1}{3}$, or $\frac{2}{3}$, in sheer desperation, and with the feeling that his judgment is nearly or quite worthless. It would be utter folly to assign weights upon a centesimal scale; and it would also be utter folly to conjecture an antecedent probability and proceed according to the method just set forth.

It is well known that the method of least squares gives very untrustworthy information in regard to the antecedent probability of large instructive errors. In regard to the other antecedent probability required for an intelligent solution of the problem, it gives no information whatever. So far as I can understand Prof. Peirce's method of arriving at a criterion, he takes two probabilities, both functions of probabilities of instructive error, and balances them against each other. This procedure reminds me of what sometimes happens in war, when two detachments of the same army meet in the dark and fire into each other, each supposing the other to belong to the common enemy. Prof. Peirce also seems to me to violate the fundamental principle of the science of probabilities, that probabilities must be independent in order that their product shall equal concurrent probability.

If a computer resorts to the criterion when he feels that his own judgment is worthless, and only then, the criterion is harmless; since it is of no importance whether a decision is made by a worthless judgment or a worthless criterion.

In the discussion that followed, Mr. A. HALL gave a brief account of the literature of the criteria which have been proposed for the rejection of doubtful observations. In addition to the criterion proposed by Prof. Peirce, which had been discussed by Mr. Doolittle, that of Mr. E. J. Stone was mentioned; and also the proofs of a criterion given by Chauvenet and Watson. The advocacy of of Peirce's criterion by Gould, Winlock, Bache, Coffin, and Schott was noticed, and also its criticism by Airy, Stone, and Glaisher, together with Glaisher's approval of De Morgan's method of treating observations. In conclusion, Mr. HALL said:

The general result of what has been done in this matter appears to be as as follows:

Every one can devise a criterion that suits himself, but it will not please other people.

Now there seems to be a good reason underlying this. The attempt to establish an arbitrary and general criterion for the discussion and rejection of observations is an attempt to eliminate from this work the knowledge and judgment of the investigator. Such an attempt ought to fail, and it certainly will fail at length, no matter by what personal influence it may be supported. It is true that no proof has been given of the principle of the arithmetical mean for a finite number of observations, such as the practical cases that always come before us; but we assume this principle as leading to the most probable result. When we depart from this principle, it must be done, I think, for reasons that are peculiar to each case, and there can be no better guide than the judgment of the investigator. It may be said that if the criteria that have been proposed be carefully managed they will do little harm, since the result of the arithmetical mean will be altered very little; and in fact this is their chief recommendation. But by diminishing the value of the real probable error the criteria give to the observations a fictitious accuracy and a weight they do not deserve.

The paper was also discussed by Messrs. HILL, ELLIOTT, FARQUHAR, WOODWARD, and others, including Mr. JAMES MAIN, a visitor—all agreeing, on essential points, with Mr. Doolittle's view.

Mr. R. S. WOODWARD then discussed

THE SPECIAL TREATMENT OF CERTAIN FORMS OF OBSERVATION-EQUATIONS.

[Abstract.]

In a set of observation-equations whose type is

$$x_0 + (t - t_0)\, y - n = v \text{ with weight } p,$$

in which t_0 is an arbitrary constant, the same for each equation, and in which the residuals, v, are supposed to arise solely from errors in the observed quantities, n, it will be best to make

$$t_0 = \frac{[pt]}{[p]}$$

This value of t_0 makes the co-efficient of y in the first normal equation and the co-efficient of x_0 in the second normal equation, zero, and hence gives directly

$$x_0 = \frac{[pn]}{[p]}$$

$$y = \frac{[p(t-t_0)n]}{[p(t-t_0)^2]}$$

The weight of this value of x_0 is a maximum; *i. e.*, the value of x_0 corresponding to $t_0 = \frac{[pt]}{[p]}$ has a greater weight than the value of x_0 corresponding to any other value of t_0.

The probable error of the function $x_0 + \mu y$ is given by the simple formula.

$$\sqrt{\varepsilon_{x_0}^2 + \mu^2 \varepsilon_y^2}$$

in which ε_{x_0} and ε_y are the probable errors of x_0 and y, respectively.

The investigation shows that, when several standards of length are to be intercompared two and two, in order to obtain the length of some one of them, it will be conducive to accuracy to have the mean temperatures of the several sets of comparisons equal.

Remarks were made upon this communication by Mr. KUMMELL.

Mr. ALEX. S. CHRISTIE made a communication on

CONTACT OF PLANE CURVES.*

[Abstract.]

Let $0 = f(x, y)$, (1), $0 = \varphi(x, y)$, (2), and $y = \psi(x)$, (3) be the equations of plane curves. Transferring the origin to (ξ, η), where $\eta = \psi(\xi)$, writing f, φ for $f(\xi, \eta)$, $\varphi(\xi, \eta)$, respectively, and u_n for $\frac{1}{n!}\frac{\delta^n f}{\delta \xi^n}$, v_n for $\frac{1}{n!}\frac{\delta^n \varphi}{\delta \xi^n}$, we have

from (1), $0 = \sum_0^\infty \left(\frac{y^n}{n!}\frac{\delta^n}{\delta \eta^n} \cdot \sum_0^\infty \left(x^r u_r\right)\right)$, (1′), from (2),

$0 = \sum_0^\infty \left(\frac{y^n}{n!}\frac{\delta^n}{\delta \eta^n}\right) \cdot \Sigma(x^r v_r)$, (2′), and from (3), $y = x\frac{d\eta}{d\xi} + \frac{x^2}{2!}\frac{d^2\eta}{d\xi^2}$

$$+ \frac{x^3}{3!}\frac{d^3\eta}{d\xi^3} + \&c. \qquad (3')$$

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* Throughout this paper, δ, for lack of sorts, is put for round d, and denotes partial differentiation.

Writing (3′) in the form $y = xw_1 + x^2w_2 + x^3w_3 + \&c.$ (3″)

and assuming $y^\nu = x^\nu(\nu_0) + x^{\nu+2}(\nu_2) + \&c.$ (4)

Where (ν_0) obviously equals w_1^ν, and (ν_1), (ν_2), &c., are functions of ξ, η to be determined, we have, from (4), $\nu y^{\nu-1}\frac{dy}{dx} = x^{\nu-1}\nu(\nu_0)$

$$+ x^\nu . \overline{\nu + 1} . (\nu_1) + x^{\nu+1} . \overline{\nu + 2} . (\nu_2) + \&c. \quad (5)$$

$$\text{from (3″), } \frac{dy}{dx} = x^0 . 1w_1 + x^1 . 2w_2 + x^2 . 3w_3 + \&c. \quad (6)$$

$$\text{from (3″, 5), } \nu\, y^\nu . \frac{dy}{dx} = x^\nu((\nu_0)\nu w_1) + x^{\nu+1}((\nu_0)\nu w_2 + (\nu_1) . \overline{\nu+1} . w_1)$$

$$+ x^{\nu+2}((\nu_0)\nu w_3 + (\nu_1) . \overline{\nu + 1} . w_2 + (\nu_2) . \overline{\nu + 2} . w_1) + \&c.$$

$$\text{from (4, 6), } \nu y^\nu . \frac{dy}{dx} = x^\nu(\nu_0)\,\nu w_1 + x^{\nu+1}\Big((\nu_0)\nu . 2w_2 + (\nu_1)\nu . 1w_1)\Big)$$

$$+ x^{\nu+2}((\nu_0)\nu . 3w_3 + (\nu_1)\nu . 2w_2 + (\nu_2)\nu . 1w_1) + \&c.$$

$$\because\ 0 = (\nu_0) . \overline{\nu - 0} . w^2 + (\nu_1) . \overline{0 - 1} . w_1$$

$$0 = (\nu_0) . \overline{2\nu - 0} . w_3 + (\nu_1) . \overline{\nu - 1} . w_2 + (\nu_2) . \overline{0 - 2} . w_1 .$$

$$0 = (\nu_0) . \overline{3\nu - 0} . w_4 + (\nu_1) . \overline{2\nu - 1} . w_3 + (\nu_2) . \overline{\nu - 2} . w_2 + (\nu_3) . \overline{0 - 3} . w_1$$

. . . .

$$0 = (\nu_0) . \overline{m\nu - 0} . w_{m+1} + (\nu_1) . \overline{(m - 1)\nu - 1} . w_m + (\nu_2) . \overline{(m - 2)\nu - 2} . w_{m-1} + (\nu_3) . \overline{(m - 3)\nu - 3} . w_{m-2} + \ldots + (\nu_m) . \overline{0 - m} . w_1$$

Whence we have—

$$(\nu_m) = \frac{(w_1)^{\nu - m}}{m!} \times$$

$$\left| \begin{array}{llllll} \nu - 0 \,.\, w_2 \,, & 0 - 1 \,.\, w_1 \,, & 0 & , \ldots 0 & & , 0 \\ 2\nu - 0 \,.\, w_3 \,, & \nu - 1 \,.\, w_2 \,, & 0 - 2 \,.\, w_1 & , \ldots 0 & & , 0 \\ 3\nu - 0 \,.\, w_4 \,, & 2\nu - 1 \,.\, w_3 \,, & \nu - 2 \,.\, w_2 & , \ldots 0 & & , 0 \\ \vdots & \vdots & \vdots & \vdots & & \vdots \\ (m-2)\nu - 0 \,.\, w_{m-1} \,, & (m-3)\nu - 1 \,.\, w_{m-2} \,, & (m-4)\nu - 2 \,.\, w_{m-3} & , \ldots & 0 - (m-2) \,.\, w_1 & , 0 \\ (m-1)\nu - 0 \,.\, w_m \,, & (m-2)\nu - 1 \,.\, w_{m-1} \,, & (m-3)\nu - 2 \,.\, w_{m-2} & , \ldots & \nu - (m-2) \,.\, w_2 & , 0 - (m-1) \,.\, w_1 \\ m\nu - 0 \,.\, w_{m+1} \,, & (m-1)\nu - 1 \,.\, w_m \,, & (m-2)\nu - 2 \,.\, w_{m-1} & , \ldots & 2\nu - (m-2) \,.\, w_3 & , \nu - (m-1) \,.\, w_2 \end{array} \right| \quad (7)$$

which determines the coefficients in (4). (*) Putting u_{rs} for $\frac{1}{r!, s!,} \frac{\delta^{r+s} f}{\delta \xi^r \delta \eta^s}$, we have $\frac{y^\nu}{\nu!,} \frac{\delta^\nu}{\delta \eta^\nu} \sum_0^\infty (x^r u_r) = x^\nu \left((\nu_0) u_{0\nu} \right) + x^{\nu+1} \left((\nu_0) u_{1\nu} + (\nu_1) u_{0\nu} \right) + x^{\nu+2} \left((\nu_0) u_{2\nu} + (\nu_1) u_{1\nu} + (\nu_2) u_{0\nu} \right) +$ &c., and this in (1′) gives an equation of the form

$$0 = x^0 A_0 + x^1 A_1 + x^2 A_2 + x^3 A_3 + \text{\&c.} \tag{8}$$

viz:
$$\begin{aligned} 0 = {} & x^0 [(0_0) u_{00}] + x^1 [(0_0) u_{10} + (0_1) u_{00} + (1_0) u_{01}] \\ & + x^2 [(0_0) u_{20} + (0_1) u_{10} + (0_2) u_{00} + (1_0) u_{11} + (1_1) u_{01} + (2_0) u_{02}] \\ & + x^3 [(0_0) u_{30} + (0_1) u_{20} + (0_2) u_{10} + (0_3) u_{00} + (1_0) u_{21} + (1_1) u_{11} \\ & + (1_2) u_{01} + (2_0) u_{12} + (2_1) u_{02} + (3_0) u_{03}] + \text{\&c.} \end{aligned} \tag{8'}$$

for the abscissae of points common to (1) and (3). Similarly for the abscissae of points common to (2) and (3) we get an equation of the form

$$0 = x^0 B_0 + x^1 B_1 + x^2 B_2 + x^3 B_3 + \text{\&c.} \tag{9}$$

viz: $0 = x^0 [(0_0) v_{00}] + x^1 [(0_0) v_{10} + (0_1) v_{00} + (1_0) v_{01}] +$ &c. (9′)

Let (2) contain at least p parameters, enabling us to pass (2) through p of the intersections of (1) with (3). When this is done we have the equation $0 = x^0 (A_0 - B_0) + x^1 (A_1 - B_1) + x^2 (A_2 - B_2) +$ &c. (10) true for the p values of x corresponding to the p points common to (1), (2), (3). Let the p common points move to the origin, (10) must have p roots equal zero, that is, $0 = A_0 - B_0$, $0 = A_1 - B_1$, $0 = A_2 - B_2$, . . . $0 = A_{p-1} - B_{p-1}$ (11)

If we suppose (3) the parabolic representative of (1), x in (8) becomes indeterminate, and hence besides $0 = A_0$ we have also $0 = A_1$, $0 = A_2$, &c.
that is, $0 = f$, with

$$\left\{ \begin{aligned} 0 &= \frac{\delta f}{\delta \xi} + \frac{d\eta}{d\xi} \frac{\delta f}{\delta \eta} \\ 0 &= \frac{1}{2} \frac{\delta^2 f}{\delta \xi^2} + \frac{d\eta}{d\xi} \frac{\delta^2 f}{\delta \xi \delta \eta} + \frac{1}{2} \frac{d^2\eta}{d\xi^2} \frac{\delta f}{\delta \eta} + \frac{1}{2} \left(\frac{d\eta}{d\xi} \right)^2 \frac{\delta^2 f}{\delta \eta^2} \\ 0 &= \frac{1}{3!} \frac{\delta^3 f}{\delta \xi^3} + \frac{1}{2!} \frac{d\eta}{d\xi} \frac{\delta^3 f}{\delta \xi^2 \delta \eta} + \frac{1}{2!} \frac{d^2\eta}{d\xi^2} \frac{\delta^2 f}{\delta \xi \delta \eta} + \frac{1}{3!} \frac{d^3\eta}{d\xi^3} \frac{\delta f}{\delta \eta} \\ & + \frac{1}{2!} \left(\frac{d\eta}{d\xi} \right)^2 \frac{\delta^3 f}{\delta \xi \delta \eta^2} + \frac{1}{2!} \frac{d\eta}{d\xi} \frac{d^2\eta}{d\xi^2} \frac{\delta^2 f}{\delta \eta^2} + \frac{1}{3!} \left(\frac{d\eta}{d\xi} \right)^3 \frac{\delta^3 f}{\delta \eta^3} \\ & \text{\&c.} \qquad \text{\&c.} \qquad \text{\&c.} \end{aligned} \right. \tag{12}$$

---

* Putting $x = 1$ in (3″) and (4), we obtain the multinomial theorem in the form
$(w_1 + w_2 + w_3 + \text{\&c.})^\nu = (\nu_0) + (\nu_1) + (\nu_2) +$ &c.

equations fully determining $\frac{d\eta}{d\xi}$, $\frac{d^2\eta}{d\xi^2}$ · $\frac{d^3\eta}{d\xi^3}$, &c., in terms of the partial derivatives of f.

Again, suppose (3) the parabolic representative of (2), then $0 = B_0$, with $0 = B_1$, $0 = B_2$, &c., and consequently by (11) $0 = A_0$, with $0 = A_1$, $0 = A_2$, . . . $0 \doteq A_{p-1}$, or the first $p - 1$ of the equations (12) are satisfied indifferently whether the $\frac{d\eta}{d\xi}$, $\frac{d^2\eta}{d\xi^2}$, . . . $\frac{d^{p-1}\eta}{d\xi^{p-1}}$ therein contained be derived from (1) or (2); that is, we have arrived at Lagrange's conditions for contact of the $(p - 1)$ order, as a consequence of p - punctual contact; and it follows at once that the distance between two curves in the neighborhood of a p - tuple common point is of the p^{th} order when the distance along the curves from the p - tuple point is of the 1st order.*

Note.

The abstracts of communications to the Mathematical Section have each been examined by a special committee, consisting of the Chairman, the Secretary, and a third member appointed by the Chairman. These third members were as follows:

Title.	*Author.*	*Third Member.*
Alignment Curves on any Surface	C. H. KUMMELL.	A. S. CHRISTIE.
The Mass of a Planet from Observations of two Satellites	A. HALL.	W. B. TAYLOR.
Infinites and Infinitesimals	M. H. DOOLITTLE.	G. W. HILL.
Planetary Perturbations of the Moon	G. W. HILL.	E. B. ELLIOTT.
The Law of Error practically tested by Target-Shooting	C. H. KUMMELL.	A. S. CHRISTIE.
Form of Least-Square Computation	H. FARQUHAR.	R. S. WOODWARD.
Rejection of Doubtful Observations	M. H. DOOLITTLE.	W. C. WINLOCK.
Special Treatment of certain forms of Observation-Equations	R. S. WOODWARD.	W. C. WINLOCK.
Contact of Plane Curves	A. S. CHRISTIE.	C. H. KUMMELL.

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* This paper will be continued.

CORRIGENDA.

Vol. V, p. 86, line 2. For "abused" read *absurd.*
" " " 7. For "east" read *earth.*

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BULLETIN

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VOL. VII.

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BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

CONSTITUTION, RULES,

LIST OF

OFFICERS AND MEMBERS,

AND REPORTS OF

SECRETARIES AND TREASURER.

CONSTITUTION

OF

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

ARTICLE I. The name of this Society shall be THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

ARTICLE II. The officers of the Society shall be a President, four Vice-Presidents, a Treasurer, and two Secretaries.

ARTICLE. III. There shall be a General Committee, consisting of the officers of the Society and nine other members.

ARTICLE IV. The officers of the Society and the other members of the General Committee shall be elected annually by ballot; they shall hold office until their successors are elected, and shall have power to fill vacancies.

ARTICLE V. It shall be the duty of the General Committee to make rules for the government of the Society, and to transact all its business.

ARTICLE VI. This constitution shall not be amended except by a three-fourths vote of those present at an annual meeting for the election of officers, and after notice of the proposed change shall have been given in writing at a stated meeting of the Society at least four weeks previously.

STANDING RULES

FOR THE GOVERNMENT OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

---

1. The Stated Meetings of the Society shall be held at 8 o'clock P. M. on every alternate Saturday; the place of meeting to be designated by the General Committee.

2. Notice of the time and place of meeting shall be sent to each member by one of the Secretaries.

When necessary, Special Meetings may be called by the President.

3. The Annual Meeting for the election of officers shall be the last stated meeting in the month of December.

The order of proceedings (which shall be announced by the Chair) shall be as follows:

First, the reading of the minutes of the last Annual Meeting.

Second, the presentation of the annual reports of the Secretaries, including the announcement of the names of members elected since the last annual meeting.

Third, the presentation of the annual report of the Treasurer.

Fourth, the announcement of the names of members who, having complied with Section 13 of the Standing Rules, are entitled to vote on the election of officers.

Fifth, the election of President.

Sixth, the election of four Vice-Presidents.

Seventh, the election of Treasurer.

Eighth, the election of two Secretaries.

Ninth, the election of nine members of the General Committee.

Tenth, the consideration of Amendments to the Constitution of the Society, if any such shall have been proposed in accordance with Article VI of the Constitution.

Eleventh, the reading of the rough minutes of the meeting.

4. Elections of officers are to be held as follows:

In each case nominations shall be made by means of an informal ballot, the result of which shall be announced by the Secretary; after which the first formal ballot shall be taken.

In the ballot for Vice-Presidents, Secretaries, and Members of the General Committee, each voter shall write on one ballot as many names as there are officers to be elected, viz , four on the first ballot for Vice-Presidents, two on the first for Secretaries, and nine on the first for Members of the General Committee; and on each subsequent ballot as many names as there are persons yet to be elected; and those persons who receive a majority of the votes cast shall be declared elected.

If in any case the informal ballot result in giving a majority for any one, it may be declared formal by a majority vote.

5. The Stated Meetings, with the exception of the annual meeting, shall be devoted to the consideration and discussion of scientific subjects.

The Stated Meeting next preceding the Annual Meeting shall be set apart for the delivery of the President's Annual Address.

6. Sections representing special branches of science may be formed by the General Committee upon the written recommendation of twenty members of the Society.

7. Persons interested in science, who are not residents of the District of Columbia, may be present at any meeting of the Society, except the annual meeting, upon invitation of a member.

8. Similar invitations to residents of the District of Columbia, not members of the Society, must be submitted through one of the Secretaries to the General Committee for approval.

9. Invitations to attend during three months the meetings of the Society and participate in the discussion of papers, may, by a vote of nine members of the General Committee, be issued to persons nominated by two members.

10. Communications intended for publication under the auspices of the Society shall be submitted in writing to the General Committee for approval.

11. Any paper read before a Section may be repeated, either entire or by abstract, before a general meeting of the Society, if such repetition is recommended by the General Committee of the Society.

12. New members may be proposed in writing by three members of the Society for election by the General Committee; but no person shall be admitted to the privileges of membership unless he signifies his acceptance thereof in writing within two months after notification of his election.

13. Each member shall pay annually to the Treasurer the sum of five dollars, and no member whose dues are unpaid shall vote at the annual meeting for the election of officers, or be entitled to a copy of the Bulletin.

In the absence of the Treasurer, the Secretary is authorized to receive the dues of members.

The names of those two years in arrears shall be dropped from the list of members.

Notice of resignation of membership shall be given in writing to the General Committee through the President or one of the Secretaries.

14. The fiscal year shall terminate with the Annual Meeting.

15. Members who are absent from the District of Columbia for more than twelve months may be excused from payment of the annual assessments. They can, however, resume their membership by giving notice to the President of their wish to do so.

16. Any member not in arrears may, by the payment of one hundred dollars at any one time, become a life member, and be relieved from all further annual dues and other assessments.

All moneys received in payment of life membership shall be invested as portions of a permanent fund, which shall be directed solely to the furtherance of such special scientific work as may be ordered by the General Committee.

STANDING RULES

OF THE

GENERAL COMMITTEE OF THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

1. The President, Vice-Presidents, and Secretaries of the Society shall hold like offices in the General Committee.

2. The President shall have power to call special meetings of the Committee, and to appoint Sub-Committees.

3. The Sub-Committees shall prepare business for the General Committee, and perform such other duties as may be entrusted to them.

4. There shall be two Standing Sub-Committees; one on Communications for the Stated Meetings of the Society, and another on Publications.

5. The General Committee shall meet at half-past seven o'clock on the evening of each Stated Meeting, and by adjournment at other times.

6. For all purposes except for the amendment of the Standing Rules of the Committee or of the Society, and the election of members, six members of the Committee shall constitute a quorum.

7. The names of proposed new members recommended in conformity with Section 11 of the Standing Rules of the Society, may be presented at any meeting of the General Committee, but shall lie over for at least four weeks before final action, and the concurrence of twelve members of the Committee shall be necessary to election.

The Secretary of the General Committee shall keep a chronological register of the elections and acceptances of members.

8. These Standing Rules, and those for the government of the Society, shall be modified only with the consent of a majority of the members of the General Committee.

RULES

FOR THE

PUBLICATION OF THE BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

---

1. The President's annual address shall be published in full.

2. The annual reports of the Secretaries and of the Treasurer shall be published in full.

3. When directed by the General Committee, any communication may be published in full.

4. Abstracts of papers and remarks on the same will be published, when presented to the Secretary by the author in writing within two weeks of the evening of their delivery, and approved by the Committee on Publications. Brief abstracts prepared by one of the Secretaries and approved by the Committee on Publications may also be published.

5. If the author of any paper read before a Section of the Society desires its publication, either in full or by abstract, it shall be referred to a committee to be appointed as the Section may determine.

The report of this committee shall be forwarded to the Publication Committee by the Secretary of the Section, together with any action of the Section taken thereon.

6. Communications which have been published elsewhere, so as to be generally accessible, will appear in the Bulletin by title only, but with a reference to the place of publication, if made known in season to the Committee on Publications.

OFFICERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON

ELECTED DECEMBER 22, 1883.

President	J. C. WELLING.	
Vice-Presidents	J. S. BILLINGS.	GARRICK MALLERY.
	J. E. HILGARD.	ASAPH HALL.
Treasurer	CLEVELAND ABBE.	
Secretaries	HENRY FARQUHAR.	G. K. GILBERT.

MEMBERS AT LARGE OF THE GENERAL COMMITTEE.

H. H. BATES.	E. B. ELLIOTT.
W. H. DALL.	ROBERT FLETCHER.
C. E. DUTTON.	WILLIAM HARKNESS.
J. R. EASTMAN.	J. J. KNOX. *

C. V. RILEY.

STANDING COMMITTEES.

On Communications:

J. S. BILLINGS, *Chairman.* HENRY FARQUHAR. G. K. GILBERT.

On Publications:

G. K. GILBERT, *Chairman.* CLEVELAND ABBE. HENRY FARQUHAR.

S. F BAIRD†

* Mr. Knox resigned May 10, 1884, and the General Committee elected Mr. F. W. Clarke to the vacancy.

† As Secretary of the Smithsonian Institution.

OFFICERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

ELECTED DECEMBER 20, 1884.

President............	ASAPH HALL.	
Vice-Presidents........	J. S. BILLINGS.	GARRICK MALLERY.
	WILLIAM HARKNESS.	J. E. HILGARD.
Treasurer............	ROBERT FLETCHER.	
Secretaries............	G. K. GILBERT.	HENRY FARQUHAR.

MEMBERS AT LARGE OF THE GENERAL COMMITTEE.

MARCUS BAKER. H. H. BATES.
F. W. CLARKE. W. H. DALL.
C. E. DUTTON. J. R. EASTMAN.
E. B. ELLIOTT. H. M. PAUL.
C. V. RILEY.

STANDING COMMITTEES.

On Communications:

J. S. BILLINGS, *Chairman.* G. K. GILBERT. HENRY FARQUHAR.

On Publications:

G. K. GILBERT, *Chairman.* ROBERT FLETCHER. HENRY FARQUHAR.
S. F. BAIRD.*

*As Secretary of the Smithsonian Institution.

LIST OF MEMBERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

Corrected to December 20, 1884.

The names of founders are printed in SMALL CAPITALS.

(*d*) indicates *deceased*.

(*a*) indicates *absent* from the District of Columbia and excused from payment of dues until announcing his return.

(*r*) indicates *resigned*.

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Abbe, Cleveland	Army Signal Office. 2017 I St. N. W	1871, Oct. 29
Abert, Sylvanus Thayer	1724 Penn. Ave. N. W	1875, Jan. 30
Adams, Henry	1607 H St. N. W	1881, Feb. 5
Aldis, Asa Owen	1765 Mass. Ave	1873, Mar. 1
Allen, James	Army Signal Office. 1907 I St. N.W	1882, Feb. 25
Alvord, Benjamin (*d*)		1872, Mar. 23
ANTISELL, THOMAS	Patent Office. 1311 Q St. N. W	1871, Mar. 13
Avery, Robert Stanton	Coast and Geodetic Survey Office. 320 A St. S. E.	1879, Oct. 11
Babcock, Orville Elias (*d*)		1871, June 9
Bailey, Theodorus (*d*)		1873, Mar. 1
BAIRD, SPENCER FULLERTON	Smithsonian Institution. 1445 Mass. Ave. N. W.	1871, Mar. 13
Baker, Frank	326 C St. N. W	1881, May 14
Baker, Marcus	Coast and Geodetic Survey Office. 1205 Rhode Island Ave.	1876, Mar. 11
Bancroft, George	1623 H St. N. W., or Newport, R. I	1875, Jan. 16
Barnard, William Stebbins	Agricultural Department. 917 N. Y. Ave. N. W., or Canton, Ill.	1884, Mar. 1
BARNES, JOSEPH K. (*d*)		1871, Mar. 13
Bates, Henry Hobart	Patent Office. The Portland	1871, Nov. 4
Bean, Tarleton Hoffman	National Museum. 1411 R. I. Ave	1884, Apr. 26
Beardslee, Lester Anthony (*a*)	Captain U. S. N., Navy Department	1875, Feb. 27
Bell, Alexander Graham	Scott Circle. 1500 R. I. Ave	1879, Mar. 29
Bell, Chichester Alexander	1221 Conn. Ave	1881, Oct. 8
BENÉT, STEPHEN VINCENT	Ordnance Office, War Department. 1717 I St. N. W.	1871, Mar. 13
Bessels, Emil	Smithsonian Institution. 1444 N St. N. W.	1875, Jan. 16
BILLINGS, JOHN SHAW	Surg. Genl's Office, U. S. A. 3027 N St. N. W.	1871, Mar. 13
Birney, William	456 Louisiana Ave. 1901 Harewood Ave., Le Droit Park.	1879, Mar. 29
Birnie, Rogers (*a*)	Cold Spring, Putnam Co., N. Y	1876, Mar. 11
Blair, Henry Wayne (*d*)		1884, Feb. 2
Bodfish, Sumner Homer	Geological Survey. 605 F St. N. W	1883, Mar. 24
Boutelle, Charles Otis	Coast and Geodetic Survey Office. 1513 20th St. N. W.	1884, Feb. 16
Bowles, Francis Tiffany	1813 Jefferson Place	1884, Mar. 29
Brown, Stimson Joseph	Naval Observatory. 2133 K St. N.W	1884, Apr. 12
Browne, John Mills	Medical Director, U. S. N. The Portland.	1883, Nov. 24
Burchard, Horatio Chapin	Director of the Mint. Riggs House	1879, May 10

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Burgess, Edward Sandford	High School. 810 12th St. N. W	1883, Mar. 24
Burnett, Swan Moses	1215 I St. N. W	1879, Mar. 29
Busey, Samuel Clagett	1525 I St. N. W	1874, Jan. 17
CAPRON, HORACE	The Portland	1871, Mar. 13
Case, Augustus Ludlow (a)	Bristol, R. I	1872, Nov. 16
CASEY, THOMAS LINCOLN	Col. Corps of Engineers. 1419 K St. N. W.	1871, Mar. 13
Caziarc, Louis Vasmer (a)	War Department	1882, Feb. 25
CHASE, SALMON PORTLAND (d)		1871, Mar. 13
Chamberlin, Thomas Crowder	Geological Survey	1883, Mar. 24
Chickering, John White, Jr	Deaf Mute College, Kendall Green	1874, Apr. 11
Christie, Alexander Smyth	Coast and Geodetic Survey Office, 628 Mass. Ave. N. W.	1880, Dec. 4
Clapp, William Henry (a)	Ft. Davis, Tex. 1416 Corcoran St. Washington.	1882, Feb. 25
Clark, Edward	Architect's Office, Capitol. 417 4th St. N. W.	1877, Feb. 24
Clark, Ezra Westcote	Revenue Marine Bureau, Treasury Department. Woodley Road.	1882, M. r. 25
Clarke, Frank Wigglesworth	Geological Survey. 1425 Q St. N. W	1874, Apr. 11
COFFIN, JOHN HUNTINGTON CRANE	1901 I St. N. W	1871, Mar. 13
Collins, Frederick (d)		1879, Oct. 21
Comstock, John Henry (a)	Cornell University, Ithaca, N. Y	1880, Feb. 14
Coues, Elliott	Smithsonian Inst. 1726 N. St. N. W	1874, Jan. 17
CRAIG, BENJAMIN FANEUIL (d)		1871, Mar. 13
Craig, Robert	Army Signal Office. 1008 I St. N. W	1873, Jan. 4
Craig, Thomas (a)	Johns Hopkins Univ., Baltimore, Md	1879, Nov. 22
CRANE, CHARLES HENRY (d)		1871, Mar. 13
Curtis, George Edward	Army Signal Office. 1416 Corcoran St.	1884, Jan. 5
Curtis, Josiah (d)		1874, Mar. 28
Cutts, Richard Dominicus (d)		1871, Apr. 29
DALL, WILLIAM HEALEY	Care Smithsonian Institution. 1119 12th St. N. W.	1871, Mar. 13
Davis, Charles Henry (d)		1874, Jan. 17
Davis, Charles Henry	Navy Department. 1705 Rhode Island Ave. N. W.	1880, June 19
Dean, Richard Crain (a)	Naval Hospital, New York	1872, Apr. 23
De Caindry, William Augustin	Commissary General's Office. 924 19th St. N. W.	1881, Apr. 30
De Land, Theodore Louis	Treasury Dept. 126 7th St. N. E	1880, Dec. 18
Dewey, Frederick Perkins	National Museum. 1007 G St. N. W	1884, Apr. 25
Dewey, George (r)		1879, Feb. 15
Diller, Joseph Silas	Geological Survey	1884, Mar. 1
Doolittle, Myrick Hascall	Coast and Geodetic Survey Office. 1925 I St. N. W.	1876, Feb. 12
Dorr, Frederic William (d)		1874, Jan. 17
Dunwoody, Henry Harrison Chase (a)	Army Signal Office. 3012 Dumbarton St., Georgetown.	1873, Dec. 20
Dutton, Clarence Edward	Geological Survey	1872, Jan. 27
DYER, ALEXANDER B. (d)		1871, Mar. 13
Earll, Robert Edward	National Museum	1884, Apr. 26
Eastman, John Robie	Naval Observatory. 1823 I St. N. W	1871, May 27
EATON, AMOS BEEBE (d)		1871, Mar. 13
Eaton, John	Bureau of Education, Interior Dept. 712 East Capitol St.	1874, May 8
Eimbeck, William	Coast and Geodetic Survey Office	1884, Feb. 2
Eldredge, Stewart (a)	Yokohama, Japan.	1871, June 9
ELLIOT, GEORGE HENRY (r)		1871, Mar. 13
ELLIOTT, EZEKIEL BROWN	Government Actuary, Treasury Department. 1210 G St. N. W.	1871, Mar. 13
Emmons, Samuel Franklin	Geological Survey. 23 Lafayette Place.	1883, Apr. 7
Endlich, Frederic Miller (a)	Smithsonian Institution. Lake Valley, New Mexico.	1873, Mar. 1
Ewing, Charles (d)		1874, Jan. 17
Ewing, Hugh (a)	Lancaster, Ohio	1874, Jan. 17
Farquhar, Edward	Patent Office Library. 1915 H St. N.W.	1876, Feb. 12

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Farquhar, Henry	Coast and Geodetic Survey Office. Brooks Station, D. C.	1881, May 14
Ferrel, William	Army Signal Office. 471 C St. N. W.	1872, Nov. 16
Fletcher, Robert	Surgeon Genl's Office, U. S. A. 1326 L St. N. W.	1873, Apr. 10
Flint, Albert Stowell	Naval Observatory. 1450 Chapin St., College Hill.	1882, Mar. 25
Flint, James Milton	Navy Dept. U. S. S. Albatross	1881, Mar. 19
FOOTE, ELISHA (d)		1871, Mar. 13
Foster, John Gray (d)		1873, Jan. 18
French, Henry Flagg (r)		1882, Mar. 25
Fristoe, Edward T.	1434 N St. N. W.	1873, Mar. 29
Gale, Leonard Dunnell (d)		1874, Jan. 17
Gallaudet, Edward Miner	Deaf Mute College, Kendall Green	1875, Feb. 27
Gannett, Henry	Geological Survey. 1881 Harewood Ave., Le Droit Park.	1874, Apr. 11
Gardiner, James Terry (a)	State Survey, Albany, N. Y.	1874, Jan. 17
Garnett, Alexander Young P. (r)		1878, Mar. 16
Gihon, Albert Leary	Naval Hospital, 2019 Hillyer Place N. W.	1880, Dec. 18
Gilbert, Grove Karl	Geological Survey. 1424 Corcoran St.	1873, June 7
GILL, THEODORE NICHOLAS	Smithsonian Institution	1871, Mar. 13
Godding, William Whitney	Government Asylum for the Insane.	1879, Mar. 29
Goode, George Brown	National Museum. 1620 Mass. Ave. N. W.	1874, Jan. 31
Goodfellow, Edward	Coast and Geodetic Survey Office	1875, Dec. 18
Goodfellow, Henry (r)		1871, Nov. 4
Gore, James Howard	Columbian Univ. 1305 Q St. N. W.	1880, Mar. 14
Graves, Edward Oziel (a)	Asst. Treasurer U. S.	1874, Apr. 11
Graves, Walter Hayden (a)	Denver, Colorado	1878, May 25
Greely, Adolphus Washington	Army Signal Office. 1909 I St.	1880, June 19
Green, Bernard Richardson	1738 N St. N W.	1879, Feb. 15
Green, Francis Mathews (a)	Navy Department	1875, Nov. 9
GREENE, BENJAMIN FRANKLIN (a)	West Lebanon, N. H.	1871, Mar. 13
Greene, Francis Vinton	District Commissioners' Office, 1915 G St. N. W.	1875, Apr. 10
Gregory, John Milton	15 Grant Place	1884, Mar. 29
Gunnell, Francis M.	Surgeon General, U. S. N. 600 20th St. N. W.	1879, Feb. 1
Hains, Peter Conover	1824 Jefferson Place	1879, Feb. 15
HALL, ASAPH	Naval Observatory. 2715 N St. N. W.	1871, Mar. 13
Hall, Asaph, jr.	Naval Observatory. 2715 N St. N. W.	1880, Dec. 20
Hanscom, Isaiah (d)		1873, Dec. 20
HARKNESS, WILLIAM	Naval Observatory. 1415 G St. N. W.	1871, Mar. 13
Hassler, Ferdinand Augustus (a)	Santa Ana, Los Angeles Co., Cal.	1880, May 8
HAYDEN, FERDINAND VANDEVEER (a)	Geological Survey. 1803 Arch St., Philadelphia, Penn.	1871, Mar. 13
Hazen, Henry Allen	P. O. Box No. 427. 1416 Corcoran St.	1882, Mar. 25
Hazen, William Babcock	Army Signal Office. 1601 K. St. N. W.	1881, Feb. 5
Heap, David Porter	Light House Board, Treasury Department. 1618 Rhode Island Ave.	1884, Mar. 15
HENRY, JOSEPH (d)		1871, Mar. 13
Henshaw, Henry Wetherbee	Bureau of Ethnology, P. O. Box 585	1874, Apr. 11
HILGARD, JULIUS ERASMUS	Coast and Geodetic Survey Office. 1709 Rhode Island Ave. N. W.	1871, Mar. 13
Hill, George William	Nautical Almanac Office. 314 Ind. Ave. N. W.	1879, Feb. 1
Hitchcock, Romyn	P. O. Box 630	1884, Apr. 26
Holden, Edward Singleton (a)	Madison, Wisconsin	1873, June 21
Holmes, William Henry	Geological Survey. 1100 O St. N. W.	1879, Mar. 29
Hough, Franklin Benjamin (a)	Agricultural Dept. Lowville, N. Y.	1879, Mar. 29
Howell, Edwin Eugene (a)	Rochester N. Y.	1874, Jan. 31
HUMPHREYS, ANDREW ATKINSON (d)		1871, Mar. 13
Jackson, Henry Arundel Lambe (a)	War Department	1875, Jan. 30
James, Owen (a)	Scranton, Pa.	1880, Jan. 3
Jeffers, William Nicolson (r)		1877, Feb. 24
JENKINS, THORNTON ALEXANDER	2115 Penn. Ave. N. W.	1871, Mar. 13

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Johnson, Arnold Burges	Light House Board, Treasury Dept. 501 Maple Ave., Le Droit Park.	1878, Jan. 19
Johnson, Joseph Taber	926 17th St. N. W	1879, Mar. 29
Johnson, Willard Drake	Geological Survey. 501 Maple Ave., Le Droit Park.	1884, Feb. 16
Johnston, William Waring	1603 K St. N. W	1873, Jan. 21
Kampf, Ferdinand (*d*)		1875, Dec. 18
Kauffmann, Samuel Hays	1000 M St. N. W	1884, Feb. 16
Keith, Reuel	2219 I St	1871, Oct. 29
Kerr, Mark Brickell	Geological Survey. 812 21st St. N. W.	1884, Feb. 16
Kerr, Washington Caruthers (*a*)	Raleigh, N. C	1883, Apr. 7
Kidder, Jerome Henry	Smithsonian Inst. 1816 N St., N. W.	1880, May 8
Kilbourne, Charles Evans (*a*)	War Department	1880, June 19
King, Albert Freeman Africanus	726 13th St. N. W	1875, Jan. 16
King, Clarence (*r*)		1879, May 10
Knox, John Jay (*a*)	Nat. Bk. Republic, New York City	1874, May 8
Kummell, Charles Hugo	Coast and Geodetic Survey Office. 608 Q St. N. W.	1882, Mar. 25
LANE, JONATHAN HOMER (*d*)		1871, Mar. 13
Lawrence, William	First Comptroller's Office, Treasury Department. 1344 Vermont Ave.	1884, Feb. 16
Lawver, Winfield Peter	Mint Bureau, Treasury Department. 1912 I St. N. W.	1881, Feb. 19
Lee, William	2111 Penn. Ave. N. W	1874, Jan. 17
Lefavour, Edward Brown	Coast and Geodetic Survey Office. 905 O St. N. W.	1882, Dec. 16
Lincoln, Nathan Smith	1514 H St. N W	1871, May 27
Lockwood, Henry H. (*r*)		1871, Oct. 29
Loomis, Eben Jenks	Nautical Almanac Office. 1413 College Hill Terrace N. W.	1880, Feb. 14
Lull, Edward Phelps (*a*)	74 Cedar St., Roxbury, Mass	1875, Dec. 4
Lyford, Stephen Carr (*r*)		1873, Jan. 18
MacCauley, Henry Clay (*a*)	P. O. Box 953, Minneapolis, Minn	1880, Jan. 3
McGee, W J	Geological Survey. 1424 Corcoran St.	1883, Nov. 10
McGuire, Frederick Bauders	1306 F St. N. W. 614 E. St. N. W	1879, Feb. 15
Mack, Oscar A. (*d*)		1872, Jan. 27
McMurtrie, William (*a*)	Champaign, Ill	1876, Feb. 26
Maher, James Arran	Geological Survey. 21 E St. N. W	1884, Feb. 16
Mallery, Garrick	Bureau of Ethnology, P. O. Box 585. 1323 N St. N. W.	1875, Jan. 30
Marcou, John Belknap	Geological Survey	1884, Mar. 29
Marvin, Joseph Badger (*a*)	Internal Revenue Bureau	1878, May 25
Marvin, Archibald Robertson (*d*)		1874, Jan. 31
Mason, Otis Tufton	National Museum. 1305 Q St. N. W	1875, Jan. 30
Matthews, Washington	Surgeon General's Office, U. S. A	1884, June 7
MEEK, FIELDING BRADFORD (*d*)		1871, Mar. 13
Meigs, Montgomery (*a*)	U. S. Engineer Office, Keokuk, Iowa	1877, Mar. 24
MEIGS, MONTGOMERY CUNNINGHAM	1239 Vermont Ave. N. W	1871, Mar. 13
Merrill, George Perkins	National Museum	1884, Apr. 26
Milner, James William (*d*)		1874, Jan. 31
Morgan, Ethelbert Carroll	918 E St. N. W.	1883, Oct. 13
Morris, Martin Ferdinand (*r*)		1877, Feb. 24
Murdoch, John	Smithsonian Institution. 1441 Chapin St., College Hill.	1884, Apr 26
Mussey, Reuben Delavan	P. O. Box 618. 508 5th St. N. W	1881, Dec. 3
MYER, ALBERT J. (*d*)		1871, Mar. 13
Myers, William (*a*)	War Department	1871, June 23
NEWCOMB, SIMON	Navy Department	1871, Mar. 13
Nichols, Charles Henry (*a*)	Bloomingdale, N. Y	1872, May 4
NICHOLSON, WALTER LAMB	1322 I St. N. W	1871, Mar. 13
Nordhoff, Charles	1731 K St	1879, May 10
Norris, Basil	1829 G St. N. W	1884, Mar. 1
Ogden, Herbert Gouverneur	Coast and Geodetic Survey Office. 1324 19th St. N. W.	1784, Feb. 2
Osborne, John Walter	212 Delaware Ave. N. E	1878, Dec. 7

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
OTIS, GEORGE ALEXANDER (*d*)		1871, Mar. 13
PARKE, JOHN GRUBB	Engineer Bureau, War Department. 16 Lafayette Square.	1871, Mar. 13
PARKER, PETER	2 Lafayette Square	1871, Mar. 13
Parry, Charles Christopher (*a*)	Davenport, Iowa	1871, May 13
Patterson, Carlile Pollock (*d*)		1871, Nov. 17
Paul, Henry Martyn	Naval Observatory. 109 1st St. N. E.	1877, May 19
Peale, Albert Charles	Geological Survey. 1010 Mass. Ave. N. W.	1874, Apr. 11
PEALE, TITIAN RAMSAY (*a*)	Philadelphia, Penn	1871, Mar. 13
PEIRCE, BENJAMIN (*d*)		1871, Mar. 13
Peirce, Charles Sanders (*a*)	Coast and Geodetic Survey Office	1873, Mar. 1
Pilling, James Constantine	Geological Survey. 918 M St. N. W.	1881, Feb. 19
Poe, Orlando Metcalfe (*a*)	34 Congress St. West, Detroit, Mich	1873, Oct. 4
Poindexter, William Mundy	701 15th St. N. W. 806 17th St. N. W.	1884, Dec. 20
Pope, Benjamin Franklin	Surgeon General's Office, U. S. A. 1309 20th St. N. W.	1882, Dec. 16
Porter, David Dixon (*r*)		1874, Apr. 11
Powell, John Wesley	Geological Survey. 910 M St. N. W.	1874, Jan. 17
Prentiss, Daniel Webster	1224 9th St. N. W.	1880, Jan. 3
Pritchett, Henry Smith (*a*)	Washington University, St. Louis, Mo.	1879, Mar. 29
Rathbone, Henry Reed (*a*)		1874, Jan. 17
Rathbun, Richard	Smithsonian Institution 1622 Mass. Ave. N. W.	1882, Oct. 7
Ray, Patrick Henry	Army Signal Office	1884, Jan. 5
Renshawe, John Henry	Geological Survey. 1221 O St. N. W.	1883, Feb. 24
Richey, Stephen Olin	732 17th St	1882, Oct. 7
Ricksecker, Eugene	Geological Survey. 1505 Q St. N. W.	1884, Feb. 16
Ridgway, Robert (*a*)	Smithsonian Inst. 1211 Va. Av. S. W.	1874, Jan. 31
Riley, Charles Valentine	Agricultural Department. 1700 13th St. N. W.	1878, Nov. 9
Riley, John Campbell (*d*)		1877, May 19
Ritter, William Francis McKnight	Nautical Almanac Office. 16 Grant Place.	1879, Oct. 21
Robinson, Thomas	Howard University. 6th St. N. W., cor. Lincoln.	1884, Jan. 19
Rodgers, Christopher Raymond Perry (*a*).	1723 I St. N. W	1872, Mar. 9
Rodgers, John (*d*)		1872, Nov. 16
Rogers, Joseph Addison (*a*)	Naval Observatory	1872, Mar. 9
Russell, Israel Cook	Geological Survey. 1424 Corcoran St.	1882, Mar. 25
Russell, Thomas	Army Signal Office. 1116 M. St. N. W.	1883, Feb. 10
Salmon, Daniel Elmer	Agricultural Dept. 1006 N St. N. W.	1883, Nov. 24
Sampson, William Thomas (*a*)	Torpedo Station, Newport, R. I.	1883, Mar. 24
SANDS, BENJAMIN FRANKLIN (*d*)		1871, Mar. 13
Saville, James Hamilton	342 D St. N. W. 1315 M St. N. W	1871, Apr. 29
SCHAEFFER, GEORGE CHRISTIAN (*d*)		1871, Mar. 13
SCHOTT, CHARLES ANTHONY	Coast and Geodetic Survey Office. 212 1st St. S. E.	1871, Mar. 13
Searle, Henry Robinson (*d*)		1877, Dec. 21
Seymour, George Dudley (*r*)		1881, Dec. 3
Shellabarger, Samuel	Room 23 Corcoran Building. 812 17th St. N. W.	1875, Apr. 10
Sherman, John	1319 K St. N. W	1874, Jan. 17
SHERMAN, WILLIAM TECUMSEH (*r*)		1871, Mar. 13
Shufeldt, Robert Wilson (*a*)	Surgeon Genl's Office, U.S.A., or Box 144 Smithsonian Institution.	1881, Nov. 5
Sicard, Montgomery (*a*)	Ordnance Bureau, Navy Department.	1877, Feb. 24
Sigsbee, Charles Dwight	Naval Academy, Annapolis, Md	1879, Mar. 1
Skinner, John Oscar	1529 O St. N. W	1883, Mar. 24
Smiley, Charles Wesley	U. S. Fish Commission, 1443 Mass. Ave. 943 Mass. Ave.	1882, Oct. 7
Smith, David	1330 Corcoran St	1876, Dec. 2
Smith, Edwin	Coast and Geodetic Survey Office. 2024 Hillyer Place.	1880, Oct. 23
Spofford, Ainsworth Rand	Library of Congress. 1621 Mass. Ave. N. W.	1872, Jan. 27

NAME.	P. O. ADDRESS AND RESIDENCE.	DATE OF ADMISSION.
Stearns, John (a)	Boston, Mass	1874, Mar. 28
Stearns, Robert Edwards Carter	Smithsonian Institution. 1226 Mass. Ave. N. W.	1884, Nov. 22
Stone, Ormond (a)	Leander McCormick Observatory, University of Virginia.	1874, Mar. 28
Taylor, Frederick William (a)	Smithsonian Institution. Lake Valley, New Mex.	1881, Feb. 19
TAYLOR, WILLIAM BOWER	Smithsonian Inst. 306 C St. N. W.	1871, Mar. 13
Thompson, Almon Harris	Geological Survey	1875, Apr. 10
Thompson, Gilbert	Geological Survey. 1448 Q St. N. W.	1884, Feb. 16
Tilden, William Calvin (a)	New York City.	1871, Apr. 29
Todd, David Peck (a)	Lawrence Observ., Amherst, Mass.	1878, Nov. 23
Toner, Joseph Meredith	615 Louisiana Ave	1873, June 7
True, Frederick William	National Museum	1882, Oct. 7
Twining, William J. (d)		1878, Nov. 23
Upton, Jacob Kendrick (r)		1878, Feb. 2
Upton, William Wirt	2d Comptroller's Office, Treasury Dept. 1746 M St. N. W.	1882, Mar. 25
Upton, Winslow (a)	Brown University, Providence, R. I.	1880, Dec. 4
Vasey, George (r)		1875, June 5
Walcott, Charles Doolittle	Geological Survey, Nat. Museum	1883, Oct. 13
Waldo, Frank (a)	Army Signal Office. Ft. Myer, Va.	1881, Dec. 3
Walker, Francis Amasa (a)	Mass. Inst. of Technology, Boston, Mass.	1872, Jan. 27
Walling, Henry Francis (a)	Geological Survey, Cambridge, Mass.	1883, Feb. 24
Ward, Lester Frank	Geological Survey. 1464 R. I. Ave. N. W.	1876, Nov. 18
Webster, Albert Lowry (a)	West New Brighton, Staten Island, N. Y.	1882, Mar. 25
Welling, James Clarke	1302 Connecticut Ave	1872, Nov. 16
Wheeler, George M. (a)	Engineer Bureau, War Department	1873, June 7
WHEELER, JUNIUS B. (a)	Lenoir, N. C.	1871, Mar. 13
White, Charles Abiathar	Geological Survey. Le Droit Park	1876, Dec. 16
White, Charles Henry	1744 G St. N. W.	1884, Mar. 1
White, Zebulon Lewis (a)	Providence, Rhode Island	1880, June 19
Williams, Albert, Jr	Geological Survey. 23 Lafayette Square.	1883, Feb. 24
Wilson, Allen D. (a)		1874, Apr. 11
Wilson, James Ormond	Franklin School Building. 1439 Mass. Ave. N. W.	1873, Mar. 1
Winlock, William Crawford	Naval Observatory. 723 20th St. N.W	1880, Dec. 4
Wolcott, Christopher Columbus (r)		1875, Feb. 27
Wood, Joseph (a)	Supt. Motive Power, Penn. Co., Fort Wayne, Ind.	1875, Jan. 16
Wood, William Maxwell (a)	Navy Department	1871, Dec. 2
Woodruff, Thomas Maher	Army Signal Office. 2020 Hillyer Place.	1884, Apr. 12
WOODWARD, JOSEPH JANVIER (d)		1871, Mar. 13
Woodward, Robert Simpson	Geological Survey, 1125 17th St. N. W.	1883, Nov. 24
Woodworth, John Maynard (d)		1874, Jan. 31
Yarnall, Mordecai (d)		1871, Apr. 29
Yarrow, Harry Crécy	814 17th St. N. W	1874, Jan. 31
Yeates, William Smith	Smithsonian Institution. 401 G St. N. W.	1884, Apr. 29
Zumbrock, Anton	Coast and Geodetic Survey Office. 455 C St. N. W.	1875, Jan. 30

Number of *founders*	44	
" members *deceased*		41
" " *absent*		62
" " *resigned*		16
" " *active*		173
Total number enrolled		292

CALENDAR FOR THE USE OF THE PHILOSOPHICAL SOCIETY,

Showing the alternate SATURDAYS for holding Meetings during the several "Seasons" from 1884–'85 to 1907–'08, inclusive.

PREPARED BY MR. E. B. ELLIOTT.

Submitted to the General Committee June 7, 1884, and ordered published.

Years.	October.	November.	December.	Years.	January.	February.	March.	April.	May.	June.
1884	11, 25	8, 22	6, 20	1885	3, 17, 31	14, 28	14, 28	11, 25	9, 23	6, 20
1885	10, 24	7, 21	5, 19	1886	2, 16, 30	13, 27	13, 27	10, 24	8, 22	5, 19
1886	9, 23	6, 20	4, 18	1887	15, 29	12. 26	12, 26	9, 23	7, 21	4, 18
1887	15, 29	12, 26	10, 24	1888	7, 21	4, 18	3, 17, 31	14, 28	12, 26	9, 23
1888	13, 27	10, 24	8, 22	1889	5, 19	2, 16	2, 16, 30	13, 27	11, 25	8, 22
1889	12, 26	9, 23	7, 21	1890	4, 18	1, 15	1, 15, 29	12, 26	10, 24	7, 21
1890	11, 25	8, 22	6, 20	1891	3, 17, 31	14, 28	14, 28	11, 25	9, 23	6, 20
1891	10, 24	7, 21	5, 19	1892	2, 16, 30	13, 27	12, 26	9, 23	7, 21	4, 18
1892	15, 29	12, 26	10, 24	1893	7, 21	4, 18	4, 18	1, 15, 29	13, 27	10, 24
1893	14, 28	11, 25	9, 23	1894	6, 20	3, 17	3, 17, 31	14, 28	12, 26	9, 23
1894	13, 27	10, 24	8, 22	1895	5, 19	2, 16	2, 16, 30	13, 27	11, 25	8, 22
1895	12, 26	9, 23	7, 21	1896	4, 18	1, 15, 29	14, 28	11, 25	9, 23	6, 20
1896	10, 24	7, 21	5, 19	1897	2, 16, 30	13, 27	13, 27	10, 24	8, 22	5, 19
1897	9, 23	6, 20	4, 18	1898	15, 29	12, 26	12, 26	9, 23	7, 21	4, 18
1898	15, 29	12, 26	10, 24	1899	7, 21	4, 18	4, 18	1, 15, 29	13, 27	10, 24
1899	14, 28	11, 25	9, 23	1900	6, 20	3, 17	3, 17, 31	14, 28	12, 26	9, 23
1900	13, 27	10, 24	8, 22	1901	5, 19	2, 16	2, 16, 30	13. 27	11, 25	8, 22
1901	12, 26	9, 23	7, 21	1902	4, 18	1, 15	1, 15, 29	12, 26	10, 24	7, 21
1902	11, 25	8, 22	6, 20	1903	3, 17. 31	14, 28	14, 28	11, 25	9, 23	6, 20
1903	10, 24	7, 21	5, 19	1904	2, 16, 30	13, 27	12, 26	9, 23	7, 21	4, 18
1904	15, 29	12, 26	10, 24	1905	7, 21	4, 18	4, 18	1, 15, 29	13, 27	10, 24
1905	14, 28	11, 25	9, 23	1906	6, 20	3, 17	3, 17, 31	14, 28	12, 26	9, 23
1906	13, 27	10, 24	8, 22	1907	5, 19	2, 16	2, 16, 30	13, 27	11, 25	8, 22
1907	12, 26	9, 23	7, 21	1908	4. 18	1, 15, 29	14, 28	11, 25	9. 23	6, 20

ANNUAL REPORT OF THE SECRETARIES.

WASHINGTON CITY, *December* 20, 1884.

To the Philosophical Society of Washington:

We have the honor to present the following statistical data for 1884.

At the beginning of the year the number of active members was 151

This number has been increased by the addition of 35 new members and by the return of 5 absent members. It has been diminished by the departure of 13 members and by the death of 5. There have been no resignations. The net increase of active members has thus been . . . 22

And the active membership is now 173

The roll of new members is:

W. S. BARNARD.
T. H. BEAN.
H. W. BLAIR.
C. O. BOUTELLE.
F. T. BOWLES.
S. J. BROWN.
G. E. CURTIS.
F. P. DEWEY.
J. S. DILLER.
R. E. EARLL.
WILLIAM EIMBECK.
ASAPH HALL, Jr.
J. M. GREGORY.
D. P. HEAP.
ROMYN HITCHCOCK.
W. D. JOHNSON.
S. H. KAUFFMANN.
M. B. KERR.
WILLIAM LAWRENCE.
J. A. MAHER.
J. B. MARCOU.
WASHINGTON MATTHEWS.
G. P. MERRILL.
JOHN MURDOCH.
BASIL NORRIS.
H. G. OGDEN.
P. H. RAY.
W. M. POINDEXTER.
EUGENE RICKSECKER.
THOMAS ROBINSON.
R. E. C. STEARNS.
GILBERT THOMPSON.
C. H. WHITE.
T. M. WOODRUFF.
W. S. YEATES.

The names of deceased members are:

BENJAMIN ALVORD. O. E. BABCOCK. H. W. BLAIR.
CHARLES EWING. J. J. WOODWARD.

There have been 15 general meetings for the presentation and discussion of papers (not including the public meeting of Dec. 6); the average attendance has been 42. There have been six meetings of the Mathematical Section; average attendance 15.

In the general meeting 32 communications have been presented; in the mathematical section 11. Altogether 43 communications have been made by 32 members and one guest. The number of members who have participated in the discussions is 38. The total number who have contributed to the scientific proceedings is 50, or 29 per cent. of the present active membership.

Very Respectfully,
G. K. GILBERT,
H. FARQUHAR,
Secretaries.

ANNUAL REPORT OF THE TREASURER.

WASHINGTON CITY, *December* 31, 1884.

To the Philosophical Society of Washington:

I have the honor to present herewith my annual statement as Treasurer for the year ending December 20th, 1884.

The revenue of the Society has amounted to $855.00 and the expenditures have been $671.96, leaving a balance of $183.04 on hand; the details of this account are given in the accompanying table.

The investments of the funds of the Society have not changed and consist, therefore, of $1,000 in a U. S. Bond at 4½ per cent. and $1,500 in U. S. Bonds at 4 per cent.

The receipts during the past year may be classified as follows:

Interest on invested fund			$95
5	Dues for 1882,	$25	
16	" " 1883,	80	
126	" " 1884,	630	
2	" " 1885,	10	
149		745	

The dues remaining unpaid are about as follows:

For 1882,	3	$15
" 1883,	10	50
" 1884,	47	235
	60	300

Early in February 500 copies of Volume VI of the Bulletin were received from the printer, and 148 copies have been distributed to active members, also 67 copies have been sent to domestic and 73 to foreign recipients; occasional copies of other volumes have also been sent to complete broken sets. The stock of publications now on hand is about as follows:

Bulletin, Volume I.	91 copies.
" " II.	82 "
" " III.	199 "
" " IV.	184 "
" " V.	201 "
" " VI.	215 "
W. B. Taylor, Memoir of Joseph Henry, 1st Ed.	64 copies.
" " " 2d Ed.	30 "
J. C. Welling, Address on life of Joseph Henry	4 "
W. B. Taylor, Address as President	70 "

In return for the distribution of Bulletins the Society has received about seventy-five publications from other organizations or individuals and the Accessions Catalogue of the Library now includes 177 titles.

Very respectfully your obedient servant,

CLEVELAND ABBE, *Treasurer.*

DR. *The Philosophical Society of Washington in account with Cleveland Abbe, Treasurer, for the year ending Dec. 20, 1884.* CR.

EXPENDITURES.

Date.	Vou'r.	Check.	To whom paid.		Amount.
1884.					
Feb. 1	1	80	H. Farquhar, expenses as Sec'y		$27 90
Feb. 1	2	81	G. K. Gilbert, " "		8 88
Feb. 1	3	82	Judd & Detweiler, printers		39 55
Feb. 6	4	83	Cleveland Abbe, exp. as Treas.		20 75
Feb. 18	5	85	A. H. Gawler, janitor		10 00
Feb. 14	6	84	Judd & Detweiler, prs.	$200 00	
April 2	6	86	" "	150 00	
July 15	6	89	" "	28 00	
Aug. 1	6	90	" "	41 07	
					419 07
May 15	7	87	G. K. Gilbert, exp. as Sec'y		5 37
June 12	8	88	H. Farquhar, " "		17 15
Aug. 1	9	91	J. C. Welling, President		50
Aug. 1	10	92	Judd & Detweiler, printers		31 60
Aug. 1	11	93	A. H. Gawler, janitor		7 00
Dec. 6	12	94	G. K. Gilbert, exp. as Sec'y		10 68
			Total		598 45
			Balance on hand		183 04
					781 49

RECEIPTS.

From what source.		Amount.	Total.
Credit by receipts as follows:			
Balance carried over from Dec. 31, '83.		----	----
(*See over-draft*, $73.51, *below*.)			
Annual dues received:			
and deposited February 2, 1884		$245 00	
" " " 9, "		155 00	
" " April 3, "		100 00	
" " June 11, "		20 00	
" " July 15, "		5 00	
" " August 1, "		95 00	
" " Dec'mb'r 16, "		90 00	
		710 00	
Cash in hand December 20, 1884		35 00	
Repayment for special printing, deposited February 2		15 00	
Interest of $1,000 at 4½	$45 00		
" 1,500 at 4	50 00		
		95 00	
			$855 00
Less over-draft of Dec., 1883		----	73 51
Total		----	781 49

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

ANNUAL ADDRESS OF THE PRESIDENT.

ANNUAL ADDRESS OF THE PRESIDENT,

JAMES C. WELLING.

Delivered December 6, 1884.

THE ATOMIC PHILOSOPHY, PHYSICAL AND META-PHYSICAL.

Every nation under the sun has a philosophy of some kind, but the philosophy we profess draws the lines of its historic traditions, if not its "increasing purpose," from the home of our Aryan ancestors in Greece. It was here that the typical forms of our literature were invented, that the art of sculpture was carried to its climax, and that the architecture of the lintel came to a transfiguration in the Theseum and the Parthenon. And as if all these glories were not enough, it is the further good fortune of the Greeks to have at least opened up the great leading problems of human enquiry, in physics, in psychology, and in ethics; and to have so opened them up at the starting point of the world's Torch-race, that the light shed on these questions more than twenty-five centuries ago is still a matter of curious retrospection to this generation of ours on whom the ends of the world are come.

It is to one of the oldest of the formal physical philosophies ever framed by the mind of man for the explanation of the mechanical structure of the Universe that I purpose to call your attention to-night—a theory the most comprehensive in its scope, and, at the same time, the most searching in its subtility, which has been handed down to us by all antiquity—a theory which in its ingenuity represents the synthetic power of the Greek mind at the highest stage of its physical speculation—a theory which the literature of Rome has preserved in the amber of Cicero's philosophical disquisition, and embalmed in the immortal verse of Lucretius—a theory, in fine, which has survived the old dialectic in which it was first conceived, because it has come to a new birth in the forms of modern science. I refer to what is known in history as the Atomic Philosophy of the Greeks.

The fundamental principle of the ancient physical philosophy—its point of departure and its ever re-entering point of return—is found in the famous well-worn maxim of metaphysics, that out of nothing nothing comes, and that what *is* can never be annihilated. It was in the name of this maxim and under the shadow of its authority that the Greek physical philosophers sought to shelter their whole right of free enquiry from the charge of impiety, and if to us the dictum seems the merest truism, it was not so regarded at the dawn of natural philosophy. Sometimes used as a logical club with which to brain a stolid and incurious indifferentism, and sometimes waved as a red flag in the face of polytheistic superstition, it meets us perpetually in all the oldest records of ancient philosophical speculation—in the formal elaborations of Aristotle,* in the lucubrations of Boëthius,† and in the verse of poets as remote from each other in style and creed as Lucretius, the lively Epicurean,‡ and Persius, the sternest of Stoic moralists.§ This maxim stirred the philosophical mind of antiquity to its lowest depth, because it was then the type and symbol of a whole method of philosophizing—a method regarded by many as not a little presumptuous, much as the Copernican theory of the Universe was regarded in the sixteenth century, or much as the Formula of Evolution is regarded to-day outside of scientific circles.

It was because the maxim seemed to so many the challenge of a vain wisdom and of a false philosophy that the early champions of physical philosophy sometimes felt themselves called to vindicate the truth of this truism by an appeal to formal argument. The necessity for such an appeal measures the scientific ineptitude of the average mind at that early age. "If what emerges into sensible perception," argues Epicurus with the utmost gravity, "can be conceived as coming from nothing, then everything might come of anything, and that, too, without any need of germs; and if what disappears from sensible perception was really destroyed into nothing, then all things might perish without anything being left into which

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* Aristotle: *De Generatione et Corruptione*, I, iii, 5, (Didot's ed., vol. 2, p. 437.)

† Boëthius: *De Consolatione Philosophiæ*, Lib. V, Prosa 1.

‡ Lucretius: *De Rerum Natura*, I, 151–227.

§ Persius: *Satira*, iii, 84.

they were resolved."* Such was the rude flint-flake with which, as their only weapon of logic, the early Nimrods of philosophy in Greece defended their right to philosophize in the palæolithic stage of natural enquiry.

As the next step in this metaphysical logic we find a distinction drawn by the ancient Greek philosophers between things as they are in substrate and things as they appear, disappear, and reappear in time—between the noumenal and the phenomenal world, as we would say to-day in the Kantian phraseology. It was the favorite doctrine of the Eleatic school of philosophers that we get a true conception of things only when, abstracting from their individuality, their partitiveness and their changing forms, we find the ultimate root and unity of all being in a simple, indivisible, and unchangeable substrate, which is the true object of knowledge, because it is the true basis of all reality. This concept increased in clearness as it passed through the minds of Xenophanes, Parmenides, and Empedocles, until, in the generalizations of the last-named philosopher, the ultimate substrate of things was resolved into four elementary substances—earth, air, fire, and water; each uncreated and imperishable, each equal in quantity, each composed, within itself, of parts that are qualitatively the same, and each forever incommutable with the others; yet each and all capable of every variety and degree of mixture in the manifold combinations of things as they appear in the sensible world.

On the other hand, it was held by Heraclitus that this fundamental substrate or unity of things is a mere figment of the philosophical imagination, and that it is only as things are conceived to be in perpetual flux that the forms of our knowledge can be brought into correspondence with the forms of actual being. That is, to the doctrine of the unchanging substrate of things Heraclitus opposed the doctrine of the perpetual flux of things.

It remained to effect a synthesis and reconciliation between these opposing views of the Eleatic and Heraclitic philosophies of nature, while at the same time saving the fundamental dogma of all natural philosophizing, that out of nothing nothing comes. Such a basis of pacification was found in the terms of the Atomic Philosophy, in the doctrine that the changing forms, positions, motions, and phases of

* Diog. Laërt.: Lives of the Philosophers, *sub voce* "Epicurus."

things are to be conceived as a perpetual flux, resulting from the changing permutations and combinations of the indestructible atoms composing the eternal substrate of nature. And thus it was that the doctrine of ultimate atoms, incessantly modified in the forms of their combination, but remaining forever the same in substance, became the legitimate deduction and the crowning corollary of the primal eldest maxim of physical philosophy. Aristotle expressly gives this genesis of the Atomic Philosophy of Greece in its reduction by Anaxagoras. After saying that Anaxagoras hypothesized an infinity of atoms, to explain the myriad varieties of nature, because he wished to avoid the reproach of getting something out of nothing, Aristotle adds: "From the fact that contraries are made out of each other, they must needs have previously existed in each other; for if everything that *becomes* must needs come either from something or from nothing, and if this latter alternative is impossible, (about which all who treat of nature are agreed in opinion,) then it only remains to infer that everything which becomes must have come from the things in which it pre-existed, though, on account of the smallness of their bulks, made out of things imperceptible to us."*

The Atomic Philosophy of the Greeks was, therefore, not a mere exhalation of the imagination, but a logical inference from the starting point and major premise of their natural metaphysics. The doctrine of ultimate atoms in nature was, indeed, the necessary complement and reconciliation of the conception that all things are in elemental stir, and that yet in this elemental stir there is no creation of anything out of nothing and no annihilation of anything, but only composition, decomposition, and recomposition.

It need not surprise us, therefore, to find that the doctrine of ultimate atoms in nature is a universal form of thought among thinking men of all the most advanced races in antiquity. Into the hidden historic springs of the Atomic Philosophy, as formulated by the Greeks, it is not here proposed to enquire. Whether its

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* Aristotle: *Naturalis Auscultatio*, I, iv, 2, (Didot's ed., vol. 2, p. 252.)

Compare, also, Lucretius, *De Rer. Nat.*, I, 543–545:

—— "*Quoniam* supra docui nil posse creari
De nilo, neque quod genitum est ad nil revocari,
Esse immortali primordia corpore *debent*."

germs were derived from Egypt, or from India, or from Phœnicia, or whether it was an original birth of the Hellenic mind, is a matter of curious historic interest which hardly admits, perhaps, of precise and positive determination, though certain it is that India had an Atomic Philosophy before the Greeks. However possible or probable it may be that the early Greek philosophers borrowed some of their lore under this head, as we know they did under others, from the Egyptian priests; or whatever truth there may be in the tradition, reported by Posidonius,* (Cicero's teacher in philosophy,) that one Moschus, a Phœnician, imparted the doctrine to Pythagoras, it is very certain that the Greek philosophers have made the doctrine their own by the logical development they gave to it, and by the hereditament in it which they have bequeathed to the subsequent generations of men moving along the lines of human progress. It has been more than suspected that the doctrine dates in Greece from the age of Pythagoras, by reason of certain specific ideas, which we can read in the spectrum analysis of the most distant times by the light of modern anthropological science. Certain definite lines of thought are to be found in the psychology of every epoch, and these lines betray the mental constitution of the epoch as surely as the vapors of the elements absorb rays of the same refrangibilities that they radiate. In the days of Pythagoras we discover certain psychical ideas which are seen to have been the natural reflex of the great fundamental dogma out of which the Atomic Philosophy sprang. I refer to the doctrine of metempsychosis and of its correlate, the pre-existence of souls. If it be assumed that the human soul is something generically different from the body, and is not generated by it, then it necessarily follows, according to the maxim *De nihilo nihil fit*, that the soul pre-existed somewhere before the atoms of the body were put together, and from the other branch of the maxim, that it must continue to exist somewhere after the body is dissolved. The doctrine of the transmigration of souls is not, therefore, a mere vagary of the ethnical imagination, but the natural offspring of that form of Pythagorean dualism which distinguished the soul, as not only generically, but genetically distinct from the body. Hence, the

*Strabo: *Geog.*, Lib. XVI. *Cf.* Sextus Empiricus: *Adversus Mathematicos*, Lib. 9.

carry with it any clear conception of personal identity, and hence Lucretius justly argued that the doctrine of a future life, as held by many in his day, was stripped of all significance if the chain of personal consciousness is broken at death.*

And to this fundamental antithesis of ideas lying at the bottom of these two forms of the Greek Atomic Philosophy another antithesis must be added in the Stratonical Hylozoism, which, assuming in matter an atomic structure partly material and partly vital, proceeded to account for the genesis of animated bodies on the superadded assumption of a plastic energy working in nature to the production of every living thing. In a word, Strato's matter, instinct with life, and waiting only for the first chance to be stuck together in the composition of plants and animals, seems to have been the metaphysical anticipation of our modern protoplasm.†

It was in opposition alike to the physics of Anaxagoras, Democritus, and Strato, that Plato reared his splendid fabric of idealism, while Aristotle, for his part, rejected the philosophy of atoms altogether, and installed in its place for centuries the doctrine of Form and Quality, and Substance and Entelechy, whatever that may mean. "If," he says, "there be no other substance beyond the substances existing in nature, then Physics is the first science; but if there be a certain substance which is immovable, then this is before body, and Philosophy is the first science." ‡ That single sentence recapitulates the whole verbal philosophy of the Middle Ages. Plato was so hostile to the hypothesis of Democritus that he never once names that philosopher in all his writings, though it is the Abderite physicist to whom he intends a disparaging allusion when in the *Timæus* he impales on the shafts of his irony "a certain philosopher of an indefinite and ignorant mind." Aristotle names him often enough, either separately or in conjunction with Leucippus, and treats the Atomic Philosophy with respect as an "invention framed to explain the transformation and birth of things—explaining birth and dissolution by the decomposition and recomposition of atoms,

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† Cicero aptly defines the antithesis of ideas between Democritus and Strato. See *Academ. Prior.*, Lib. II, xxxviii, 121. Also, *De Nat. Deor.*, Lib. I, xiii, 35.

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But it is in the physical philosophy of Epicurus, as that philosophy has been expounded and expanded by Lucretius, that we can discover the fullest and clearest exposition of the doctrine of atoms, considered as a key to the structure of the Universe. We here have the doctrine formulated into a theodicy of naturism, a theory of psychology, a cosmogony, and an anthropology. According to Epicurus, in his Lucretian rendering, atoms are minute material particles, indivisible, not by reason of their smallness, but of their solidity which makes them indestructible and unchangeable in their constitution; they have size, weight, and shape, yet are forever invisible to the eye; in shape, some of the atoms are different from the others, but, while the number of the different shapes is finite, the number of atoms of each shape is infinite; every atom must have at least three *cacumina* (γωνίας), that is, infinitesimally small bounding points which are incapable of existing apart from the atom, but must be conceived to coexist with it in order to give definition to it and to enclose its "solid singleness;" some of the atoms are hook-shaped, some only slightly jagged, some smooth, &c.; atoms are in incessant motion, racing through space in all directions under the stress of their weight,† according to the favoring conditions of a vacuum more or less complete, yet so that the sum of their motions results in the supreme repose of gross matter, except when a thing exhibits the motion of translation in space—a form of motion which is molar and not atomic; atoms move besides at an enormous uniform speed, in parallel lines, up and down, so far as there can be any up and down in a universe equally boundless in all directions, and except so far as some of the atoms have originally a shape which makes them capable of slight deflections from parallel straight lines—that *clinamen principiorum* which was invented by Epicurus to explain the phenomena of so-

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It is no part of my present purpose to carry the history of the Atomic Philosophy into Roman speculation. The Romans took all their ideas in mental, moral, and physical philosophy at second-hand from the Greeks.† Strong in the practical arts of war and polity, they were content to be in literature imitators and in philosophy eclectics. Equally inept for the deft metaphysical analysis of the Greeks and for their fine artistic synthesis, the Romans none the less contributed, on the practical side of life, to the definite exposition of the contents of all the philosophical systems of the Greeks. Hence we could ill spare the ponderous banter of Cicero when he mocks at the weak points of the Atomic Philosophy,‡ and still less could we spare that reasoned elaboration of its strong points which has made the *De Rerum Natura* of Lucretius the most systematic, the most complete, the most earnest, and the most realistic of all the reductions which the Atomic Philosophy has ever received. But after allowing for all his skill in the episodical handling of the rival systems of Heraclitus, Empedocles, and Anaxagoras, for his power of description, for the vivacity of his narrative, for the force and often the beauty of his illustrations and analogies, it must still be conceded that there is much more of original poetry than of original philosophy in these glowing hexameters of the Epicurean philosopher-poet.

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that time were not discussing the concourse of atoms, fortuitous or otherwise, but were carefully pondering, with Doctors Divine and Angelical, Subtile and Irrefragable, the difference between *Ens* and *Essentia*, between *materia quomodolibet accepta* and *materia signata*, between *quidditas per se* and *hæcceitas per se*, between *ultima entitas entis* and *ultima actualitas formæ*. As we plod our weary way through the *Quodlibeta* of these venerable doctors, we can but envy the angels one of the faculties ascribed to them by St. Thomas Aquinas—that of being able to pass from point to point without passing through intermediate spaces.

Bacon,* as he stood at the threshold of the new dispensation of physical science, had made a plea for the forgotten philosophy of Democritus, but when the metaphysical philosophy of Europe came to a new Avatar in the brain of Descartes, we find that thinker denying a discrete conception of matter, and arguing for the contrary conception of continuous extension, of the identification of extension with substance, and, hence, of the infinite divisibility of matter. He says: "It is easy to demonstrate that there cannot be atoms; that is, parts of bodies or of matter which are of an indivisible nature, as some philosophers have imagined, since, however small we may suppose these parts, inasmuch as they must needs have extension, we conceive that there is not one of them which cannot still be divided into two or more still smaller parts; whence it follows that it is divisible." † It will here be seen that Descartes falls into a confusion of ideas with regard to the atoms of the ancient philosophers. They did not conceive that the atom was indivisible because of its smallness, but because of the indestructible solidity which made it incapable of being cut, or broken, or bent, and which also made it impervious to heat or humidity. ‡

* See, especially, *Cogitationes de Natura Rerum*, and *De Principiis atque Originibus*, &c. Works, (Ellis & Spedding's ed., London,) vol. III, pp. 15, 82, *et seq.*; *cf.* Advancement of Learning, Book II, vii, 7, (Ellis & Spedding's ed.,) vol. III, p. 358.

† For a formal criticism on Democritus' theory of atoms see *Principes de la Philosophie*, *Œuvres de Descartes*, (Cousin,) tome III, p. 516, and *cf.* Aristotle: *De Generatione et Corruptione*, I, ii, 11–21, where this criticism is anticipated and surpassed.

‡ "*Corpora individua propter soliditatem*," Cic., De Fin., I, vi, 17; *cf.* Lucret., I, lines 582–5.

far enough away from the Epicurean atoms, but we are still working with the atoms of pure metaphysics.

It is equally in accordance with the chronological order of time, and the logical order of scientific ideas, that we should next turn to Newton. And of Newton, the greatest name in all physical philosophy, it need only be said that in his work on Optics he returned to a conception of atoms, which, except that it proceeds on the assumption of a Deity and of final cause, is substantially identical with that of Leucippus, Democritus, and Epicurus. He says: "All *these* things considered [that is, the chemical facts he had just recited], it seems probable to me that God in the beginning formed matter in solid, massy, hard, impenetrable, movable particles, of such sizes and figures, and with such other properties and in such proportion to space as most conduced to the end for which He formed them; and that these primitive particles, being solids, are incomparably harder than any porous bodies compounded of them, even so very hard as never to wear or break in pieces—no ordinary power being able to divide what God himself made one in the first creation." This definition reminds us of Lucretius.

In continuation Newton adds: "While the particles continue entire they may compose bodies of one and the same nature and texture in all ages; but should they wear away or break in pieces, the nature of things depending on them would be changed. Water and earth composed of old worn particles would not be of the same nature and texture now with water and earth composed of entire particles in the beginning. And, therefore, that nature may be lasting, the changes of corporeal things are to be placed only in the various separations and new associations, and motions of these permanent particles."

The very form of this last-cited statement carries us back to the cradle of the Atomic Philosophy.* But it is not so much the form of Newton's statement which excites our admiration as the connection of thought in which it stands. The whole of

* *Δημόκριτος δὲ καὶ Λεύκιππος ποιήσαντες τὰ σχήματα, τὴν ἀλλοίωσιν καὶ τὴν γένεσιν ἐκ τούτων ποιοῦσι, διακρίσει μὲν καὶ συγκρίσει γένεσιν καὶ φθοράν, τάξει δὲ καὶ θέσει ἀλλοίωσιν.* Aristotle: *Περι Γενεσεως καὶ Φθορας*, I, 2, 4. (Didot's ed., vol. 2., p. 434.)

that time were not discussing the concourse of atoms, fortuitous or otherwise, but were carefully pondering, with Doctors Divine and Angelical, Subtile and Irrefragable, the difference between *Ens* and *Essentia*, between *materia quomodolibet accepta* and *materia signata*, between *quidditas per se* and *hæcceitas per se*, between *ultima entitas entis* and *ultima actualitas formæ*. As we plod our weary way through the *Quodlibeta* of these venerable doctors, we can but envy the angels one of the faculties ascribed to them by St. Thomas Aquinas—that of being able to pass from point to point without passing through intermediate spaces.

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the "31st Query," under which this passage occurs in the book of "Opticks," is occupied with certain chemical analyses which Newton had made in his laboratory. Newton, we know, was an alchemist, and spent laborious days and nights in trying to discover the secret by which base metals might be rendered noble; but I can hardly concur with Prof. Jevons when he says that Newton's "lofty powers of deductive investigation were wholly useless" in the conduct of these experiments.* There is some gold at the bottom of even his alchemical crucible. He was the first to put the conception of atoms in their rightful logical connection with the phenomena of practical chemistry.†

It would here be in order to follow Joseph Boscovich in his profound theory of the constitution of matter, if in doing so we might not fall into the danger of drifting too far from the atom considered as a minim of corporeal singleness. With him the atom is a point of attractive and repulsive forces rather than an ultimate physical element; and as it was really the atom of chemical physics which Democritus posited in his mind without knowing it, thus setting up the altar of science to an "unknown god," it is time that we should hasten towards the epoch when Chemistry came to rend the vail from the face of this Isis whom the Greek atomists had so long and so ignorantly worshipped.

It is in the writings of the Hon. Robert Boyle, pleasantly described by his Irish biographer, with a somewhat Irish collocation of ideas, as " Father of Chemistry and brother of the Earl of Cork," that we find the period of transition, when the old order of metaphysical atoms is changing to give place to the new order of physical atoms as weighed and measured by modern chemistry. In his essay on "The Intestine Motions of the Particles of Quiescent Bodies," ‡ as also in his essays on Fluidity and Firmness, he threw out some positive ideas on the old atomic philosophy. He supposes it to be of Phœnician derivation, and even tries to effect a reconciliation between that philosophy and the Cartesian notion of continuous substance by drawing on the *materia subtilis* of the French philosopher (which was conceived to pass constantly, like a

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‡ Robert Boyle's Works, vol. I, p. 444.

called attention at the time to "the theory of the process," he does not seem to have apprehended the generality of the principle of definite and multiple proportions till a few years later, when the doctrine dawned on him in the course of some investigations into the constitution of olefiant gas and carburetted hydrogen gas.*

Richter, before him, had ascertained the quantity of any base required to saturate one hundred measures of sulphuric acid, and had formed a table exhibiting the proportions of the acids and alkaline bases constituting neutral salts, but Dalton took this table and translated it into the relative weights of the ultimate atoms composing these saline compounds.†

The doctrine of atomic weights had thus already become a working hypothesis in chemistry, no longer an idle speculation, and we soon find Berzelius writing to Dalton that "multiple proportions are a mystery without it." ‡

From this time onward the history of chemistry has been studded with fresh confirmations of the new atomic logic, while ever and anon prophetic glints of truth, implicit in every true physical hypothesis, have leaped into the light of ocular demonstration with each advancing stage in chemical science. Time would fail to tell the beads of the atomic rosary. The doctrine of fixed, multiple, and volumetric combinations, as formulated by Avogadro in 1813; § the determination of the proportions in which bodies combine according to the number and disposition respectively of their molecules, as announced by Ampère in 1814, with special reference to the clear-cut distinction between molecules and their integrant atoms, (already presaged before Ampère by Laurent and Gerhardt;) || the relation between the atomic weights of bodies and their specific heats, conjectured by Dalton and established by Dulong and Petit in 1819;¶ the law of isomorphism, announced by Mitscherlich at the close of the same year, from which it appeared that "a similar atomic constitution determines not only

*Henry: Memoirs of the Life and Scientific Researches of John Dalton, p. 80.

† *Ibid.*, p. 85.

‡ *Ibid.*, p. 100.

§ Wurtz: The Atomic Theory, p. 36.

|| Annales de Chimie, vol. 90, p. 43.

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† *Ibid.*, p. 85.

‡ *Ibid.*, p. 100.

§ Wurtz: The Atomic Theory, p. 86.

|| Annales de Chimie, vol. 90, p. 43.

¶ Wurtz: The Atomic Theory, p. 52.

the analogy of chemical properties, but also the similarity of physical forms;" * the discoveries in electrolysis, with their bearing on atomicity, as published by Faraday in 1834, in the Seventh Series or his Experimental Researches; † the labors of Berzelius in clarifying the atomic weights of the elements; the "law of Octaves," announced by Newlands in 1865, according to which the elements were divided into groups, having numbers differing by seven, or some multiple of seven; ‡ the enlarged Periodic System of the elements, as published by Mendelejeff in 1869, with the prognostication of undiscovered metals required to make the system complete—among them a metal which the Russian chemist proceeded to name "ekaaluminium" in advance of its discovery;§ the discovery of the missing metal in 1875, by Lecoq de Boisbaudran, who found it in a blende from the mines of Pierrefitte, in the Pyrenees, and gave to it the name of "Gallium," without knowing that he had lighted on the "missing link" of Mendelejeff; ‖ the extension of this periodic system by Lothar Meyer, with his Curve of the Elements, showing that the ductility, fusibility, and volatility of bodies are functions of their comparative atomic weights; the periodic system, as revised and extended during this very year, by Prof. Carnelley, in the light of the experimental boiling and melting points and heats of formation of the halogen compounds of the elements,¶ (chlorides, bromides, and iodides;) Carnelley's tables of color relations in chemical compounds as indicating the influence of atomic weights;** and, lastly, Carnelley's new reduction of the periodic system of the elements considered in the light of their occurrence in nature, with the helpful inferences to be drawn from it ***—these, and such like discoveries as these, following in the wake of the modern atomic

* Experimental Researches in Electricity, vol. I, pp. 230–258.

† Wurtz: The Atomic Theory, p. 58.

‡ Newlands: The Discovery of the Periodic Law, &c., p. 14.

§ *Annalen der Chemie und Pharmacie, Supplement Band* 8, p. 133 *et seq.*

‖ *Comptes Rendus*, t. LXXXI, p. 493. How fully Mendelejeff recognized in gallium the characters wanted to fill the gap in his periodic system, see *Comptes Rendus*, same volume, p. 969.

¶ Philosophical Magazine for July, 1884.

** Phil. Mag. for August, 1884.

*** Phil. Mag. for September, 1884

theory, have abundantly vindicated its value as an instrument of chemical research, while conspiring to vindicate its truth by giving to its votaries that ability of prediction which is the crucial test of science. The theory, besides, has sometimes "snatched a grace beyond the reach of art" by working retroactively to the purification of chemical method from errors and defects incident to the most careful manipulations of the practical chemist.

Standing in the presence of chemical science, as now constituted, Baron Liebig has expressed the opinion that we can scarcely conceive how it could have been developed without the Daltonian hypothesis. And yet the atom of Dalton, considered in its relation to our natural senses, is just as incapable of visible and tangible demonstration as the atom of Democritus. For this reason it is known that Faraday could never fully reconcile himself to the modern doctrine of atoms.* But, in fact, there is a genetic and a generic difference between the ancient and the modern conception. The former is the offspring of the philosophical imagination toying with analogy. The latter is the offspring of the philosophical imagination gendering with the homologies of reason. The atom of Democritus sprang into thought under the plastic forms by which he figured to himself at will the invisible relations and constitution of matter. The atom of Dalton sprang into thought from a rigid mathematical mind figuring to itself certain determinate relations which had become visible in elastic fluids. The atom of Democritus was, by the terms of its genesis, incapable of verification. The atom of Dalton was, by the terms of its genesis, capable of verification, if true, in all the gases of nature. Metaphysic thought born of the analogical reason can never conclusively prove its legitimacy. Metaphysic thought born of the homological reason can always prove its legitimacy, and, until it does, has no rights of heirship in the kingdom of science. The essential quality of a metaphysico-physical hypothesis is that it should be plausible; the essential quality of a physico-metaphysical hypothesis is that it should be apodictic. The former is "magistral and peremptory;" the latter is "ingenuous and faithful." The former is contrived in such sort as to be "soonest believed," the

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* Faraday: Experimental Researches in Electricity, vol. 2, p. 284. But *cf.* vol. I, p. 249.

latter is contrived in such sort as to be "easiliest examined," to cite the words of Bacon.*

The Atomic Philosophy may, therefore, be said to offer a good type of all that is valid in physical metaphysics, and of all that is invalid in metaphysical physics. As the child in the infantile stage of his development dwells delightedly amid fays and talismans, because his metaphysic is stronger than his physics, so the savage man, artless child of nature, is easily pleased with the rattle of some lying legend, or tickled with the straw of some preposterous myth—the more preposterous the better. A cultivated race whose imagination is creative and artistic, but whose reason has not yet been developed by the processes of a rigorous logic, will demand, as has been already said, an artful and curious felicity in their physical theories—but they will demand nothing more, because when this demand is met, their highest intellectual demand has been met. It is not until "the heir of all the ages" has learned to change the organon and method of his physical enquiries, and to put his reason over his imagination, by making imagination the hand-maid of reason, that Science is born. Long before this stage has been reached the children of Science may come to the birth, but there is not strength to deliver, because the true *maieutic* of science—experimentation with rational hypothesis, and rational hypothesis with experimentation—has not yet come to the teeming mind of philosophy. The goddess Experimentation is the Lucina of Science. The free surrender of all metaphysical conceptions to the hands of this Lucina, with the distinct knowledge that she will strangle them if they are not well formed, is the birth-pang of the scientific spirit. Until this stage of mental evolution is reached we shall have as many theories of the Universe as we have stages of culture, for every stage of culture will have a physics of its own, because it has a metaphysic of its own. Hence, the endless varieties of cosmology—the Hottentot physics, the Indian physics, the Stoical physics, the Epicurean physics, the Leibnitzian physics, the Cartesian physics, and such like—all the coinage of the metaphysical imagination. Grote enumerates as many as twelve distinct physical philosophies which divided speculative opinion in Greece during the century and a half between Thales and the Peloponnesian war.

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* The Advancement of Learning, Book I, v, 9.

It is the mission of science to bring the physics of the world into unity by reading the phenomena of the world in the dry light of reason, and by continuing to spell and parse the hieroglyphs of Nature until the rational processes of our logic are brought into demonstrated correspondence with the actual processes of Nature. Science still keeps metaphysic in her service. But instead of weaving whole fabrics from the metaphysical loom and devising ingenious tissues which only reveal the nakedness of reason, Science in passing from the known to the unknown employs metaphysic as the gossamer spider employs the single thread on which she sways and balances her movements between two solid points. The thread is tied to something solid as the condition of reaching something solid after her aerial flight. So the man of science, working in and under the limitations of physics, works on the lines of metaphysic thought when he frames the tentative hypotheses with which he returns again to the patient, practical study of nature.*

The scientific man reads the Universe backward by the inductive syllogism, because Nature has proceeded forward in her evolutions, according to an unbroken chain of antecedent causes. The physical Universe is indeed a fasciculus of natural syllogisms colligated into the compactest unity, and so holding all things, forces, and functions under the bonds of logic. The scientific man, at any given stage of his enquiry, has before him only the conclusions or at best only the minor premises and the conclusions of this world-process. And he knows that these conclusions of the natural syllogistic process have been reached through a perpetual flux in the universal complex of things, forces, and functions—a flux which dates from the beginning of star-mist and nebula, or from the beginning of that more elementary fluid out of which star-mist and nebula were generated, according to the scientific metaphysic of the present day. Is it any wonder, then, that many of the major premises of Nature's physical syllogisms should still be wrapt in impenetrable mystery to us, as many of the major premises which

* Bacon's oft-quoted contrast between metaphysicians, who, he says, spin "laborious cobwebs of learning," like spiders, and physical philosophers, who "work according to the stuff, and are limited thereby," seems hardly fair to the spider. *Advancement of Learning*, Book I, iv, 5.

we have spelled out were wrapt in an impenetrable mystery to the Greeks in the 5th century before Christ?

As there is a needs be that much of metaphysic thought must be blended with the psychological processes which lead to every passage from the known to the unknown, so every great discovery of the physical philosopher tends to widen the metaphysical horizon within which he works. The world was never so full of metaphysic as it is to-day, when physical science is transforming the minds of men not so much by the secular boons it is dropping in the lap of modern civilization as by its underlying doctrines; and these doctrines are often the mere metaphysical reflex or obverse of the physical truths they subtend. The psychological processes of every age are conditioned by its logical method, and its logical method is justified to itself by its metaphysic—by those necessary conceptions and fundamental relations which it takes to be architectonic of the Universe. What, for instance, can be more metaphysical than the latest conception of our highest physical science—the conception of vortex atoms moving in an imaginary frictionless fluid where the origin and the end of the motion are equally inconceivable? Or, take Mr. Darwin's doctrine of hypothetical gemmules "inheriting innumerable qualities from ancestral sources, circulating in the blood and propagating themselves, generation after generation, still in the state of gemmules, but failing to develop themselves into cells because other antagonistic gemmules are prepotent and overmaster them in the struggle for points of attachment" *—in what respect is this doctrine one whit less metaphysical than St. Augustine's doctrine of original and hereditary sin? Or, when the late Prof. Clifford tells us that "the Universe consists entirely of mind-stuff;" that "matter is a mental picture, in which mind-stuff is the thing represented," and that "reason, intelligence, and volition are properties of a complex which is made up of elements themselves not rational, not intelligent, not conscious"—how does his "mind-stuff" differ from the "mind-stuff" of Pythagoras, † except in the

* Galton: Hereditary Genius, p. 367; *cf.* Darwin: Animals and Plants under Domestication, (London,) vol. 2, p. 402. For a criticism on this physiological doctrine, see Encyclopædia Britannica, ("Atoms,") vol. 3, p. 42.

† For the "mind-stuff" of Pythagoras, see Cicero, *De Nat. Deorum*, I, xi, 27. For the "mind-stuff" of Clifford, see "Mind," January, 1878, p. 66.

greater ingenuity and method of the metaphysic art with which it is conceived?

If within the limits of this discussion I had the time, and if, under the limitations of my knowledge, I had the ability, to carry this enquiry into the realm of molecular physics and dynamics, where such star-eyed mystagogues as a Clausius or a Rankine, a Clerk-Maxwell or a Sir William Thompson have borne the thyrsus of science before us, it would be easy to show that, under their guidance, we have escaped the pitiless parallel lines of the Epicurean atoms only to find ourselves inextricably implicated in the knottedness and linkedness of the vortex rings of atoms as they execute their infinite evolutions and involutions, vibrating now in one period and now in another behind that vail of matter where they can be descried only by the shadowy lines they reveal to the spectroscopic imagination. "It is the mode of motion," says Clerk-Maxwell, "which constitutes the vortex rings, and which furnishes us with examples of that permanence and continuity of existence which we are accustomed to attribute to matter itself. The primitive fluid, the only true matter, entirely eludes our perceptions when it is not endued with the mode of motion which converts certain portions of it into vortex rings, and thus renders it molecular."*

Of these vortex rings we must say, in the dialect of the schools, *cognoscendo ignorantur, sed ignorando cognoscuntur.* Withheld from positive conception, yet necessitated to scientific thought and speculation by the exigencies of the knowledge we *can* conceive positively, they afford a good illustration of the physical metaphysic which has wafted the scientific mind of the present generation into an empyrean as much higher than the empyrean of Plato as the spectroscopic vision of modern science is more far-reaching than the highest flight of metaphysic wit among all the physical atomizers who ever lived or dreamed in Greece. Every chemical atom, says Sir John Herschel, is forever solving differential equations, which, if written out in full, might belt the earth. "An atom of pure iron," says Jevons, "is probably a vastly more complicated system than that of the planets and their satellites."

Between metaphysical physics and physical metaphysics there is a world-wide difference. The invisible ether posited behind the

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* Encyclopædia Britannica, *sub voce* "Atom."

vail of matter by the East Indian philosophy of the Upanishads, or by the visionary dialectic of Cleanthes, was posited there by metaphysical physics. The invisible fluid posited by modern science behind the vail of matter is posited there by physical metaphysics. The vortices of Democritus as well as the vortices of Descartes are the creations of metaphysical physics. The vortices of Helmholtz and of Sir William Thompson are the creations of physical metaphysics. The fixed and crystalline sphere of the old Ptolemaic astronomers was an invention of metaphysical physics. The solid ether which transmits to us the light of the stellar Universe, and which, as Sir John Herschell remarks, is the modern "realization of the ancient idea of the crystalline orb," is the invention of physical metaphysics. When Lucretius finds in the iridescent hues of the peacock's tail, as it shimmers in the sun, a fresh type and instance of Nature's prodigality in the display of atoms, he does but yield another contingent to the barren store of his metaphysical physics. When Dr. John Tyndall finds in the iridescences of the common soap bubble a proof that stellar space is a plenum filled with a material substance that is capable of transmitting motion with a rapidity that would girdle the equatorial earth eight times in a second, he does but yield another contingent to the fertile store of his physical metaphysics. When Dr. George Cheyne, of Scotland, expressed the opinion in the last century, that "all animals, of what kind soever, were originally and actually created at once by the hand of Almighty God, it being impossible (he said) to account for their production by any laws of mechanism;" and when he further held that "every individual animal has, *in minimis*, actually included in its loins all those who shall descend from it, and every one of these again has all its offspring lodged in its loins, and so on *ad infinitum*," and that "all this infinite number of animalcules may be lodged in the bigness of a pin's head,"* he preached a biological doctrine which sounds in the terms of metaphysical physics. When Mr. Darwin in his provisional theory of Pangenesis assumes the existence of the gemmules which inherit innumerable qualities from ancestral sources, and which prelude as gemmules that struggle for existence which antedates and therefore conditionates the terms of the human struggle witnessed in society, commerce, and national life, he expounds a biological doctrine which sounds just as clearly in the terms of physical metaphysics. When old

* J. Brown: Locke and Sydenham, p. 270.

Heraclitus proclaimed that the Universe with all it contains sprang into being from elemental heat, and was destined to be resolved again into the elemental heat from which it sprang, and thus in a ceaseless round to continue the cycle of being, he taught a doctrine of conservation and correlation of energy which had its root in metaphysical physics. When Dr. John Tyndall declares that "all our philosophy, all our poetry, all our science, all our art—Plato, Shakespeare, Newton, and Raphael—are potentially in the fires of the sun," and so tucks away the genius of a Darwin in the folds of a nebular blastema, he teaches a doctrine of equivalence which has its root in physical metaphysics.

It will thus be seen that under the dominion of Science the world has use for as much metaphysic as ever before, but only for a metaphysic radically different from the old metaphysic in its point of departure as also in the tests of its validity, and, therefore, radically different in the tenure by which it is held. The votaries of the old metaphysical physics proceeded from what was unknown to explicate and explain the known appearances of things, and rested content in explanations which seemed to consist with those appearances. The votaries of the modern physical metaphysics proceed from what is known to explicate and explain what is unknown in the deeper relations of things, and rest content in explanations only so long as, and so far as, they seem consistent with experimental proofs or with the broadest homologies of the deductive reason.

When the law of simple multiples in chemical combinations was given to the world by Dalton, and was expressed by him in atomic language, he had really made a great departure from the physical methods of Democritus, though it is curious to observe that there is a perfect identity between the metaphysical ideas underlying his logic and the metaphysical ideas of his Greek predecessor. The method of each proceeds on the assumption of the indestructibility of matter, and it is from this platform that the English chemist reaches out his hand to the Greek philosopher in token of a common metaphysic. "No new creation or destruction of matter," wrote Dalton, in his celebrated paper on "Chemical Synthesis," "is within the reach of chemical agency. We might as well attempt to introduce a new planet into the solar system, or to annihilate one already in existence, as to create or destroy a particle of hydrogen." * Democritus knew nothing of hydrogen, but he saw as clearly and said as plainly as Dalton that the antecedent premise of all physical philosophy must be found in the metaphysical maxim that "out of nothing nothing comes, and that nothing which is can ever be annihilated." †

And this maxim, with which the old Greek philosophy began, is about all of solid and sound that remains to us from the physical philosophizing of the ancients. It is true, as Mr. Balfour Stewart remarks, that the ancients had in some way grasped the idea of the essential unrest and energy of things; that they had the idea ot small particles or atoms as the constituent elements of matter, and divined the existence of an ethereal medium extending through all space; but there is no evidence at all to support the statement that any one or all of these doctrines proceeded from even a rudimental conception of "the most profound and deeply seated ot the principles of the material universe."

There is, however, one respect in which it may be justly said that Democritus stands at the head of the long line of natural philosophers who since his day have been explicating for us the structure of the physical universe. He was the first who ever attempted a purely mechanical solution of the problem of physical being. It is the singular glory of the atomic philosophers that alone, among the jarring schools of Greece, they saw that a science of the Universe was possible only on the assumption that the phenomena of the physical universe are bound together by necessary law, and this law mechanical in the modes of its operation. They had no *science*, it is true, in the modern sense of the word, but it is no small distinction which they have won in standing at the head of an intellectual succession which embraces in its ranks a Copernicus and a Galileo, a Newton and a Laplace, a Dalton and a Faraday. ‡

* Henry: Memoirs, &c., of Dalton, p. 88.

† Diog. Laert., *sub voce* "Democritus," where it is particularly recorded that he assumed as his point of departure the maxim "Out of nothing nothing comes," &c.

‡ "Was die Atomiker von ihren Vorgängern unterscheidet, ist nur die Strenge und Folgerichtigkeit mit der sie den Gedanken einer rein materialistischen und mechanischen Naturerklärung durchgeführt haben; diese kann ihnen aber um so weniger zum Nachtheil gedeutet werden, da sie damit nur die Schlüsse gezogen haben welche durch die ganze bisherige Entwicklung gefordert, und wozu in den Annahmen ihrer Vorgänger die Vordersätze gegeben waren." Zeller: *Philos. d. Griechen*, Erster Theil, 765.

With two short lessons cited to point the moral of this long story, and I have done. The first of these moralities shall be a warning against the folly of the old atomists in attempting to philosophize beyond the conditions of their knowledge. They reared imposing fabrics in astronomy, in physics, in psychology, and in anthropology, but they built without laying their foundation in any deep knowledge of nature, and laid the successive courses of their system-building in the untempered mortar of an incoherent logic. And the moral needs to be pointed as much for the admonition of modern scientific workers, with their cheap and easy cosmologies, as for the reproach of the old physiologers of Greece. One of our poets has sung:

From an old English parsonage
  Down by the sea,
There came in the twilight
  A message to me.
Its quaint Saxon legend,
  Deeply engraven,
Hath, as it seems to me,
  Teaching from heaven;
And all through the hours
  The quiet words ring,
Like a low inspiration,
  "Doe the nexte thynge."

The message is as full of inspiration for guidance in physical philosophizing as for guidance in moral conduct. *Tantum series iuncturaque pollet.*

The only other morality which time permits to be pointed at the end of this review is a warning against intellectual impatience—not that intellectual impatience rebuked by the maxim just cited, and which seeks to leap at a single bound the limitations of knowledge in any given age—but the intellectual impatience which cavils at the short-comings of the men who dug the first ditches and planted the first hedges around the vineyards of science. They were humble pioneers, but they opened the way into that land of Beulah where the men of science sit to-day beneath their own vines and fig-trees, with none to make them afraid. Even after John Dalton had come to place the key of the new Atomic Philosophy in the hands of men, it was a saying of Mitscherlich that it took fourteen years to discover and establish a single fact in chemistry. Let us not wonder, then, that it took more than two thousand years to perfect the doctrine of atoms as a clew to the "mystery of matter." Democritus invented a mechanical key of wonderful ingenuity, but it would not unlock anything that could not be unlocked without it. Newton divined that the key must be fitted to the two great wards of chemical attraction and chemical repulsion, but still the key would not turn in the adamantine lock of Nature. Dalton found that the secret of the combination must be sought in wards nicely graduated according to certain fixed, definite, and multiple numbers, and, since his day, door after door in the chemist's "chamber of imagery" has seemed to swing open at the touch of this talisman. And even, if in the next two thousand years, or in the next twenty years, the theory of John Dalton should be absorbed in some deeper truth, there will still be room in the pantheon of science for the memorial bust of the plain Manchester arithmetician, so long as men recall how far that little candle, which he lighted with inflammable gas obtained in the rudest way from the ponds of Lancashire, has thrown its quickening beams across the whole tract of modern chemistry.

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

GENERAL MEETING.

BULLETIN

OF THE

GENERAL MEETING.

244TH MEETING. JANUARY 5, 1884.

The President in the Chair.

Twenty-eight members and guests present.

The Chair announced the death, since the last meeting, of General A. A. HUMPHREYS, one of the founders of the Society.

Mr. J. R. EASTMAN made a communication on

THE ROCHESTER (MINNESOTA) TORNADO,

describing the ground as it appeared a few days after the storm, and showing that the phenomena did not indicate cyclonic motion. All disturbed objects were thrown in essentially the same direction, and were pressed down rather than lifted.

Mr. ELLIOTT related that twenty-five years previous he had crossed a storm-track consisting of a double line of fallen timber, with an interval in which the timber was standing. Mr. EASTMAN thought this phenomenon should be referred to two separate cyclones, possibly moving as companions.

Mr. DALL described storm tracks in the Escanaba region in which the trunks of prostrate trees pointed uniformly in one direction, the path of destruction being definitely limited at the margins.

Mr. E. FARQUHAR suggested that a highly inclined storm axis might account for the uniformity in the direction of the wind in the zone of destruction.

Mr. W. H. Dall read a paper on

RECENT ADVANCES IN OUR KNOWLEDGE OF THE LIMPETS,

summarizing the researches of Spengél on the sensory organs or osphradia; Cunningham on the renal organ and renopericardial pore in *Patella* and *Patina;* Fraissé on the eye in *Patina, Fissurella* and Haliotis, and the speaker on the presence of an intromittent male organ in *Cocculina.* He stated that among the *Acmæidæ* and *Patellidæ* the type of eye differs, and while in *Patina* it is of a very rudimentary character, in other genera it might be well developed, as, for instance, in *Ancistromesus*, which has as well developed eyes as *Fissurella.* He also alluded to the gradual progress in classification afforded by anatomical investigation during the past few years, and observed that nearly all the known forms except *Propilidium* and *Scutellina* were amenable to classification; our ignorance of the branchiæ in the former, and the dentition in the latter, operating to prevent a final classification in these two cases, until more is known. Those authors who study the embryology and histology usually from a single species, generally ignore the wide differences of adult anatomy between the genera of Limpets, and sow their generalizations on a basis of classification which is little in advance of that of Lamarck and his immediate successors.

Professor C. H. Hitchcock being present was invited by the Chair to address the Society, and responded briefly.

The President of the Society then pronounced a brief eulogy on General Humphreys, characterizing him as a man who had left behind him an honorable name as well for his distinguished achievements in science and in war as for the virtues and graces which adorned his private life. Mingling among his fellow-men with the utmost unobtrusiveness, and as gentle in spirit as he was brave in conduct, he brought the highest intelligence as well as the highest conscientiousness to the discharge of all the duties—scientific, military, and administrative—with which he filled his long and useful life: a life fitly closed by the serenity and peace of his beautiful death.

245TH MEETING. JANUARY 19, 1884.

The President in the Chair.

Forty-five members and guests present.

The Chair read a letter from the Biological Society of Washington inviting the members of the Philosophical Society to attend its meeting of January 25th, for the purpose of listening to the annual address of its President, Dr. C. A. White.

Announcement was made of the election to membership of Messrs. GEORGE EDWARD CURTIS and PATRICK HENRY RAY.

Mr. I. C. RUSSELL made a communication on

THE EXISTING GLACIERS OF THE HIGH SIERRA OF CALIFORNIA.

[Abstract.]

During the summer of 1883 I had an opportunity of tracing to their sources some of the ancient glaciers of the High Sierra in the region between Mono Lake and the Yosemite Valley.

From the glacial records seen during a number of excursions into the mountains it was evident that the High Sierra had formerly been so deeply covered with ice that only the culminating peaks and ridges escaped the general glaciation. From the vast névé of the mountain tops flowed long winding rivers of ice, both to the eastward and westward through the cañons and valleys. In nearly all cases the glaciers occupied drainage lines of pre-glacial date, which they modified and enlarged, but, with the exception of the cirques about the higher peaks and crests, they failed to originate any of the more prominent topographical features of the range. The glaciers of the Sierra Nevada were not connected with a northern ice-sheet, but were of local origin and of the same type as the Swiss glaciers of the present day, but of far greater magnitude. If the cañons and valleys of the Sierra are traced upward, it is almost invariably found that they head in cirques or amphitheaters, in some of which small glaciers still linger—perhaps remnants of the mighty ice-rivers that formerly flowed from the same fountains.

The first glacier visited by the writer was on the northern side of Mt. Dana, at an elevation of about 11,500 feet above the sea, and at the head of a deep cañon which drains into Leevining creek, one of the tributaries of Mono Lake. The Mt. Dana glacier is approximately 2,500 feet long and of somewhat greater breadth. Although small, and in fact but a "pocket edition" of what may be seen on a far grander scale in many mountains, yet it is a veritable glacier, with nearly all the features that characterize such ice-bodies in other countries. The distinction between the snow-ice of the névé and the more solid blue or greenish-blue ice of the glacier proper is clearly marked—as was observed to be the case also in a number of neighboring glaciers. An irregular open fissure crosses the head of the névé, corresponding to the "bergschrund" of the Swiss glaciers, while a number of parallel fractures on the border of the glacier at the foot of the snow-field form marginal crevasses with walls of solid blue ice. Near the terminus of the glacier alternating sheets of porous, white ice, and of more compact bluish ice were observed, which produce a distinct laminated or ribboned structure. Dirt-bands were plainly visible, sweeping in undulating lines across the surface of the glacier; and similar bands are a conspicuous feature in nearly all the ice-bodies seen in the High Sierra. About the foot of the Mt. Dana glacier a true terminal moraine is now in process of formation. The fall of stones and dirt from the ice onto the moraine was noticed many times during our visits. Some of the rounded stones from beneath the ice are battered and scratched and have evidently received these markings within the past few years.

On the northern side of Mt. Lyell another glacier was visited, which is the source of the Tuolumne river. The Mt. Lyell glacier is somewhat larger than the one on Mt. Dana, and, like it, exhibits characteristic glacial phenomena. A protrusion of compact, banded ice from beneath a snow-field at the head of an amphitheatre was here again observed, as well as the presence of moraines, crevasses, dirt-bands, etc. On the lower portion of this glacier were observed "ice-pyramids" of the form represented in the figure on the following page.

At the northern base of a pyramid there invariably occurs a stone or a mass of dirt, that is depressed below the general surface of the glacier, as is indicated in the sketch. The pyramid invariably points toward the noon-day sun. Its mass is composed of porous and banded ice, like that forming the general surface of the glacier, but its northern face is sheeted with compact, bluish ice. The northern face is also concave, as represented in the sketch, and usually conforms to some extent with the shape of the stone at its base.

FIG. 1. An Ice-Pyramid.

On another glacier, discovered at the head of Parker creek, one of the tributaries of Mono Lake, all the glacial phenomena mentioned above are well displayed, and, in addition, "glacier-tables" were observed in considerable numbers. The following figure represents several of the glacier-tables of the Parker creek glacier, grouped for convenience of illustration:

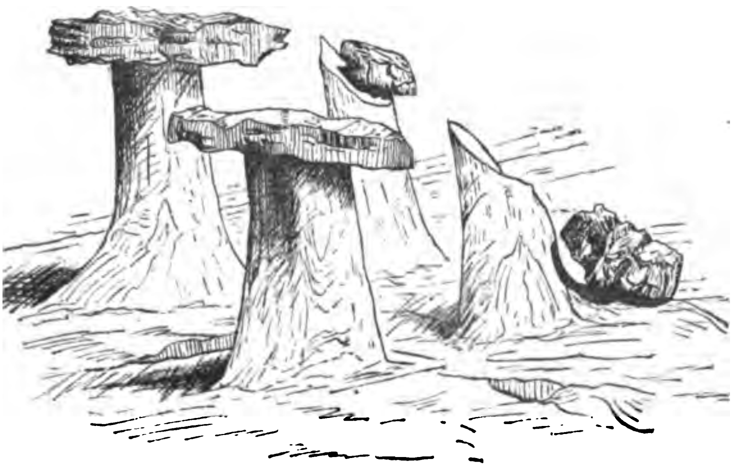

FIG. 2. Glacier-Tables.

The largest perched-block now being carried along by this glacier measures 34 by 28 by 10 feet, and is supported on a column of ice five or six feet thick, eight feet high on its northern side, and six feet high on its southern. Many masses of rock larger than the one measured were seen in the terminal moraine that circles about the foot of the glacier.

The motion of these glaciers was not observed, but that it exists is manifest from the nature of the crevasses and the curvature of the dirt-bands. The rate of flow of a glacier on Mt. McClure was measured several years since by Mr. Muir, who found it to be 47 inches in 46 days (from August 21st to October 6th, 1872).*

Six glaciers are known to the writer within the southern rim of the hydrographic basin of Mono Lake, and about twice this number were seen about Mt. Conness, Mt. McClure, Mt. Lyell, Mt. Ritter, and the Minarets.

Many of the glaciers mentioned above have been previously reported in popular articles by Mr. John Muir, but the fact that they are true glaciers having been denied by eminent geologists, it is desirable to have a more accurate description of them.

[The communication was illustrated by photographic lantern views. Its subject-matter will be more fully presented in the Fifth Annual Report of the United States Geological Survey.]

Mr. Gilbert Thompson described certain glaciers on Mount Shasta believed to be new to science. Their discovery increases the number of known glaciers on the flanks of Shasta to seven.

Mr. Holmes described modern glaciers of the Rocky Mountains observed by himself. Those of the Wind River Mountains are from one-fourth mile to one mile in length. He illustrated by a sketch the position of three small glaciers in the gorges of Mount Moran, in the Teton Range, at an altitude of 10,000 feet.

Mr. Powell remarked that the chief interest of these small modern glaciers lies in the fact that they illustrate the process by which the drift has been distributed, and aid in completing the theory of the ancient glaciation of the country.

Mr. Mark B. Kerr mentioned the occurrence of a probable glacier in the Salmon Mountains, a division of the Coast Range.

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* American Journal of Science, Vol. V, p. 69; 1873.

Mr. HARKNESS set forth the apparent difficulty of discriminating between a névé and a glacier proper, and requested that some geologist would define the term "glacier."

Mr. EMMONS said that a true glacier is an ice river, conforming in shape to the more or less restricted channel in which it flows, and this characteristic might form a base of distinction between the true glacier and the névé-field, the latter being comparable to the lake which forms the source of a mountain stream. Thus the névé would become a glacier only when from a broad and shallow ice-field it had become compressed into a narrower and deeper mass, between confining walls.

Other remarks were made by Messrs. E. FARQUHAR, GILBERT, DALL, and ELLIOTT.

Professor W. C. KERR made a communication on

THE MICA MINES OF NORTH CAROLINA.

[Abstract.]

The profitable mines are restricted to a plateau limited eastward by the Blue Ridge and westward by the Smoky Range. These were anciently worked on a very extensive scale. Few other modern mining operations have been so profitably conducted as those at the points occupied by the early miners. The ancient work was performed with blunt-pointed tools—doubtless of stone—and was confined to the partially decomposed portions of the granite veins, but large pits were nevertheless excavated. One of these measures 150 by 75 feet, and, despite a partial filling with débris, retains a depth of 35 feet. Facts connected with the arboreal vegetation show that some, and perhaps all of these openings were abandoned as much as five hundred years ago. The modern industry began in 1868, and, although it has assumed considerable importance, is not yet conducted in a systematic way.

The character of the mica and its associated minerals were discussed and illustrated by specimens.

246TH MEETING. FEBRUARY 2, 1884.

The President in the Chair.

Forty-eight members and guests present.

The Chair announced the election to membership of Mr. THOMAS ROBINSON.

Mr. C. V. RILEY made a communication on

RECENT ADVANCES IN ECONOMIC ENTOMOLOGY.

The paper set forth the part which insects play in the economy of nature, and particularly their influence on American agriculture. The earlier writers on applied entomology in the United States, as Peck, Harris, Fitch, Walsh, LeBaron, Glover, did some excellent work in their studies of the habits and life-histories of injurious species, but the most important results followed when such studies were combined with field work and experiment by competent persons and upon scientific principles. A number of the remedies proposed in the agricultural press are foolish and based on misleading empiricism. Economic entomology as a science is of comparatively recent date. It implies full knowledge of the particular injurious species to be dealt with and of its enemies, of its relations to other animals and to wild and cultivated plants. In short, the whole environment of the species must be considered, especially from the standpoint of the farmer's wants. The habits of birds, more particularly, and the bearings of meteorology and of the development of minute parasitic organisms must be considered. Experiments with insecticides and appliances will then be intelligent and successful in proportion as the facts of chemistry, dynamics, and mechanics are utilized.

The complicated nature of the problem was illustrated by the life history of *Phylloxera vastatrix* Planchon, and the difficulties often encountered in acquiring the facts were illustrated by the late work on *Aletia xylina* (Say).

The chief insecticides considered for general use and applicable above ground were tobacco, white hellebore, soap, arsenical compounds, petroleum, and pyrethrum; those for use under ground, naphthaline, sulpho-carbonate of potassium, and bisulphide of carbon. The most advantageous and improved methods of utilizing each were indicated. Recent experiment showed that kerosene emulsions, such as had been recommended lately in the author's official reports, are superior to bisulphide of carbon when used under ground against the Grape *Phylloxera*, and the discovery is deemed of great importance, especially to the French people and those on our Pacific slope. Contrary to general belief, pyrethrum powder was shown to have a peculiar and toxic effect on higher animals as well as on the lower forms of life. Its deadly influence on lower organisms led the author to strongly recommend its use as a disinfectant, and to express the belief that it will yet come to be used in medicine. Dr. H. A. Hagen's recommendation of the use of yeast ferment was touched upon. It has proved of little or no practical avail, and some of the publications on the subject were characterized as unscientific. The use of malodorous substances as repellants, which was much relied on in the early days of economic entomology and strongly recommended by the two Downings, has lately been agitated as a new principle for the prevention of insect attack by Prof. J. A. Lintner. The principle can be applied in exceptional cases to advantage, but experiment gives little hope of its utility against most of our worst field insects. Prof. S. A. Forbes is engaged in interesting researches, having for object the utilization of micro-organisms, but with more promise for pure than applied science.

Of recent progress in mechanical appliances, the paper dealt with those lately perfected under the author's direction by Dr. W. S. Barnard, one of his assistants. This part of the subject was illustrated by models and by plates from the forthcoming fourth report of the United States Entomological Commission.

The paper concluded with the following plea for applied science: "Matters of fact do not tend to provoke thought and discussion; and I must confess to some misgivings in bringing these practical considerations before a body which reflects some of the highest and purest science and philosophy of the nation. From the days of Archimedes down to the present day there has existed a disposition to decry applied science and to sneer at the practical man. Yet I often think that science, no matter in what fine-sounding name we clothe her, or how high above the average understanding we stilt her, is, after all, but common sense employed in discovering the hidden secrets of the universe and in turning them to man's wants, whether sensual or intellectual. Between the unbalanced vaporings of the pseudo-scientific theorizer and the uninformed empiric who stumbles upon a discovery, there is the firm middle ground of logical induction and deduction, and true science can neither be exalted by its inapplicability, nor degraded by its subserviency to man's material welfare. The best results follow when the pure and the applied go hand-in-hand—when theory and practice are wedded. Erstwhile the naturalist was honored in proportion as he dealt with the dry bones of his science. Pedantry and taxonomy overshadowed biologic research. To-day, largely through Charles Darwin's influence, we recognize the necessity of drawing our inspiration more directly from the vital manifestations of nature in our attempt to solve some of the many far-reaching problems which modern science presents. The fields of biology, morphology, physiology and psychology are more inviting than formerly. Nor is the lustre that glorifies the names of Stevenson, Watts, Faraday, Franklin, Morse, Henry, Siemens, and a host of yet living investigators dimmed because they made science useful. Goethe makes Wagner say:

"'Ach wenn man so in sein Museum gebannt ist
Und sieht die Welt kaum einen Feiertag
Kaum durch ein Fernglas, nur von Weiten
Wie soll man sie durch Ueberredung leiten?'

"If to-day, right here in Washington, there is great activity in the field of original research; if the nation is encouraging it in a manner we may well be proud of, the fact is due in no small degree to the efforts of those, many of them members of this Society, who have made practical ends a means, rather than to those who would make science more exclusive, and who are indifferent to practical ends or popular sympathy. Such, at least, is my apology for the nature of this paper."

In response to an inquiry by Mr. White, Mr. RILEY said that the ox-eye daisy had been subjected to a thorough test under his supervision and the result had shown that it has none of the insecticide qualities of pyrethrum.

Mr. S. M. Burnett made a communication entitled

WHY THE EYES OF ANIMALS SHINE IN THE DARK.*

[Abstract.]

Erroneous opinions have been held and expressed, not only by the non-scientific, but also by some persons holding high positions in the scientific world, in regard to the phenomena of luminosity of the eyes of animals, and particularly of cats, when they are in obscurity. It is not due, as has been commonly supposed, to phosphorescence, but to light reflected from the bottom of the eye, which light is diffused on account of the hypermetropic condition that is the rule in the lower animals.

In response to a question by Mr. White, Mr. Burnett said that human eyes affected by hypermetropia do not yield similar results, partly because the human pupil is too small and partly because the bottom of the human eye is not so strongly reflecting a surface as that of most animals.

Mr. Harkness remarked that in determining the degree of divergence of rays emitted by an eye, from an image situated upon its retina, it is necessary to consider the magnitude of that image as well as its distance from the focal plane of the lens. The divergence of the rays coming from any *one* point of the image is determined by the interval which separates the retina from the focal plane of the lens, while the divergence of the rays coming from any *two* points of the image depends principally upon the size of the image itself. The total divergence is the sum of the divergences produced by these two causes, and the neglect of that due to the size of the image will probably account for the discrepancy between the observed angle of divergence and that computed by Dr. Burnett.

It also seems desirable to bear in mind the distinction between fluorescent and phosphoresent light; the former disappears as soon as the incident waves are cut off; the latter does not.

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* This paper is published in full in the Pop. Sci. Monthly for April, 1884; Vol. XXIV, pp. 813-818.

Mr. A. B. Johnson made a communication on

SOME ECCENTRICITIES OF OCEAN CURRENTS.

[Abstract.]

The records of the Light House Board show that no less than eleven buoys of various patterns have gone adrift from the waters of the United States and been found at distant points where ocean currents have carried them. Many of these were not so fully identified that their precise original station could be indicated. In the case of a few, it has been determined that they were swept from the harbor and bay of New York by the outgoing ice in the winter of 1880–81 when nineteen buoys were carried to sea.

1. In the spring of 1871, a buoy was picked up on the west coast of Ireland.

2. In March, 1871, the Norwegian vessel Vance picked up a buoy in lat. 42° 22′, long. 26° 38′.

3. In February, 1881, a buoy went ashore on one of the cays near Turk's island. This was recognized as a New York buoy.

4. May 17, 1881, the steamer William Dickinson passed a whistling buoy in lat. 29° 46′, long. 77° 38.

5. In March, 1881, a buoy of the largest size, likewise referred to New York, was found near Bermuda.

6. In February, 1882, a Sandy Hook buoy was found near Bermuda.

7. In February or March, 1882, a buoy was washed ashore at Pendeen Cove, Penzance Bay, England.

8. In the spring of 1882, the Swedish bark Abraham Lincoln picked up a buoy in lat. 32° 30′, long. 28° 40′.

9. October 22, 1883, a buoy was picked up on the east side of Teneriffe in lat. 28° 21′, long. 16° 15′.

10. October, 1883, a second buoy was picked up fifteen miles from the east coast of Teneriffe.

11. August 20, 1883, the British bark Jane Richardson picked up a buoy in lat. 24° 11′, long. 32° 43′.

All were identified as the property of the United States by letters cast in the plates.

The charted currents of the ocean readily explain the courses and account for the positions of many of these buoys, but others appear anomalous.

Mr. JENKINS cited an instance of a bell-buoy, carried away from the coast of the United States in 1850, which was seen and heard while adrift and finally stranded on the southwest coast of Ireland.

Mr. WELLING suggested that the phenomena might not be referable to ocean currents exclusively, but in part to wind currents. Mr. JOHNSON judged from the forms of the buoys that their movements would be controlled more by currents than by winds.

Mr. H. FARQUHAR and Mr. JENKINS were of opinion that the buoy picked up off Florida might have been carried there by the southward coast-current. Mr. DALL concurred, but thought it also possible that it had made the entire circuit of the Sargasso sea.

Mr. DALL, referring to Mr. WELLING'S suggestion, said that wind and current worked together, and their effects could not be discriminated. The wind does not blow prevailingly in any direction without coercing currents to correspondence.

247TH MEETING. FEBRUARY 16, 1884.

The President in the Chair.

Fifty-four members and guests present.

The Auditing Committee reported through its Chairman, Mr. C. A. WHITE, that it had examined the accounts of the Treasurer for 1883, finding the same properly vouched in respect to expenditures and receipts. On motion of Mr. DUTTON, the report was accepted.

The Chair announced the election to membership of Mr. HENRY WAYNE BLAIR and Mr. HERBERT GOUVERNEUR OGDEN.

Mr. F. W. CLARKE made a communication on

THE PERIODIC LAW OF CHEMICAL ELEMENTS.

After giving an account of the law as worked out by Newlands, Mendelejeff, and Lothar Meyer, he exhibited an enlarged copy of

Meyer's atomic volume curve, drawn with the latest values for both atomic weight and specific gravity. On the same sheet was also drawn a similar curve, illustrating the connection between atomic weight and melting point, and it was shown that in the latter the highest portions correspond to the lowest depressions in the atomic volume curve. The opinion was expressed, in view of the regularities exhibited by these curves, that the elements had originated by some method of evolution, and that a future transmutation of one element into another was not improbable.

In reply to a question by Mr. Farquhar, Mr. CLARKE said that search was being made for similar evidence of system in the spectra of the elements, but that the subject was rendered difficult by reason of the fact that not all the lines of the spectra fall within the range of visibility.

Mr. ANTISELL remarked that while the determination of the atomic weights of the elements was one of the most important labors which the modern chemist could be occupied with until a final constant numerical result should be arrived at, and until the other properties of matter which appear to have some definite relation with the atomic weight were rigidly investigated, there was necessity for continued effort to search into those hidden relations; but if by such investigation it was believed that we could arrive at any certainty about atoms, their form and structure, or about matter itself, we should be much disappointed. Situated as we are on a cold planet, we are precluded from ever arriving, by the study of matter from a standpoint merely terrestrial, at any ideas of the ultimate nature of atom or molecule, or whether there be really any such thing as "elements" or one form of matter wholly distinct from another. To arrive at a knowledge of matter, pure and simple, we must have ready means for dissociating all compound matter, and we have at our command at present no such methods or apparatus on this globe. Subjection to intense heat is required, and our most glowing furnaces and the arc light itself is insufficient for the purpose. It calls for the exhibition of such heat as is produced in the sun and its atmosphere to reduce our elements, as we term them, to the more simple condition of matter as it exists under solar temperature, and the present spectroscope and its future improvements by which such dissociation is to be studied. The investigations of Huggins and Lockyer and other spectroscopists have revealed to us the presence of several of our so-called elements in the solar atmosphere; but constant observation has raised in the minds of these observers grave doubts whether the spectral lines of the elements, as obtained by observation of them in our atmosphere, are universally of such or whether only conditionally so, that is true only in our cold atmosphere. Doubts have arisen as to the spectral lines of elements being permanent characters of their essential nature, seeing that the spectral lines of an element, which at one time resemble those of copper, are found to be interchangeable and attached to a different element, as calcium, and that there are elements which possess the character of giving multiple spectra, as carbon, for example, which, under these solar temperatures, yields no less than three distinct and characteristic spectra.

In view of these apparently contradictory and confusing results, obtained by the examination of matter found in the solar atmosphere, which are so different from those obtained from matter in our own atmosphere, it behooves us to be very cautious in asserting the existence of any distinct elements so called, or whether there be only one matter under various cosmical conditions.

Other remarks were made by Messrs. DOOLITTLE and WHITE.

Mr. H. A. HAZEN made a communication on

THE SUN-GLOWS,

opposing the theory that they are due to dust, either cosmic or volcanic, and advocating a theory involving electrical action in connection with frost particles.*

A general discussion followed, in which Messrs. ELLIOTT, PAUL, ROBINSON, HALL, DUTTON, GILBERT, and E. FARQUHAR, participated.

Mr. ELLIOTT advocated the electrical origin of the glows, basing his argument on the simultaneousness of the phenomena throughout the planet, on the transparency of the glow as shown by observations on Lyræ, and on the extraordinary abundance of sun spots for the past few weeks.

* This paper is published in full in the American Journal of Science for March, 1884; Vol. XXVII, pp. 201–212.

248TH MEETING. MARCH 1, 1884.

The President in the Chair.

Forty-two members present.

The Chair announced that Messrs. CHARLES OTIS BOUTELLE, GILBERT THOMPSON, WILLARD DRAKE JOHNSON, and EUGENE RICKSECKER had been elected to membership.

It was announced from the General Committee that standard time would hereafter be recognized in the opening and closing of the meetings.

Mr. R. D. MUSSEY read a paper entitled

THE APPLICATION OF PHYSICAL METHODS TO INTELLECTUAL SCIENCE.

The aim of the paper was to show in how far those methods which had been successfully employed in the investigation of the phenomena of nature, and which were denominated the Physical Sciences, were applicable to those sciences, the subject-matter of which were mental operations and their results, and which, for distinction, might be named the Intellectual Sciences. Some illustrations were given of the application of these methods to the study of the law; and the paper concluded with the remark that its writer desired it to be regarded as a suggestion rather than a solution of the problem stated: "How far and in what way physical methods and physical sciences help thinkers to say *Therefore*."

Remarks were made by Mr. ROBINSON.

Mr. I. C. RUSSELL made a communication on

DEPOSITS OF VOLCANIC DUST IN THE GREAT BASIN.

[Abstract.]

In contrast with the aridity of the Great Basin at the present time, geologists have shown that during the Quaternary it was crowded with lakes. In studying the sedimentary deposits of one of these fossil lakes, named Lahontan by Mr. King, I found strata of white, unconsolidated, dust-like material, which is undistinguishable in general appearance from pure diatomaceous earth. Beds of this material, varying in thickness from a fraction of an inch to four or five feet, were observed at a number of localities in the sides of the cañons that have been carved in lacustrine strata of Lahontan age by the Humboldt, Truckee, Carson, and Walker rivers. Deposits identical with those of the Lahontan sections were observed at a number of localities among the mountains of Nevada and California at an elevation of several hundred feet above the former level of Lake Lahontan and at a distance of forty or fifty miles from its borders, thus showing that the deposits were both sub-aerial and sub-aqueous in their mode of accumulation. Further exploration revealed the fact that similar beds occur abundantly in Mono Lake Valley, where they may be seen to pass into well-characterized fragmental deposits of pumice and obsidian, thus suggesting that the finer material was also of volcanic origin. Experiment confirmed this hypothesis. Under the microscope the dust from a number of widely separated localities was found to consist almost wholly of angular flakes of transparent glass, with scarcely a trace of crystallized matter. When a sample of pumice from near Mono Lake was reduced to a fine powder, it was found to present the same physical and optical properties as the dust in question. with which it also agreed closely in chemical composition, as shown by analyses made by Dr. Chatard, of the Geological Survey.

The Mono Craters, from which this dust is supposed to have been erupted, form a group of cones about fifteen miles in length, situated in the southeastern part of the Mono Lake Valley, California. These extinct volcanoes are composed almost entirely of pumice and obsidian, in the condition both of coulées and lapilli, the latter constituting cones of great symmetry and beauty, the grandest of which have an elevation of nearly three thousand feet above Mono Lake. Some of these craters were in eruption during Quaternary times, while others were active after the ancient lakes and glaciers of the region had passed away. Many times during their history vast quantities of lapilli and dust were thrown out. As the volcanic dust interstratified with the sediments of Lake Lahontan is undistinguishable from that deposited in the Mono Basin, there is little room for doubting that they had a common origin. The greatest distance from the Mono Craters at which the dust was observed, was in the Humboldt Cañon, about two hundred miles northward of the point of eruption.

At three localities in the Lahontan Basin the bones of extinct mammals were found closely associated with the deposits described above, thus furnishing the suggestion that the showers of fine volcanic dust were, at least to some extent, fatal to animal life.

Mr. ANTISELL said it was useless to look for the source of volcanic dust in existing volcanoes on the land. Pumice in the character of fine particles, as exhibited, is exclusively the product of submarine eruption. Other remarks were made by Mr. HARKNESS.

Mr. LESTER F. WARD read a paper entitled

SOME PHYSICAL AND ECONOMIC FEATURES OF THE UPPER MISSOURI SYSTEM,

in which he described the process by which the valleys of the Lower Yellowstone and Upper Missouri are formed, and pointed out the importance and the feasibility of utilizing the water of these rivers for purposes of irrigation.*

Mr. GILBERT said that Mr. Ward's description of the process by which the Missouri constructs its flood plain was verified by a nearly identical group of phenomena observed by himself on the lower course of the Colorado. Mr. ELLIOTT concurred with the speaker's view that the system of irrigation should be inaugurated by national action rather than local. Mr. RILEY was of opinion that the proposed plan of irrigation was entirely feasible, and said that the final solution of the grasshopper problem lay in the cultivation of the northern plains.

Mr. BURCHARD said that while the political advantage of a continuous belt of settlement uniting the Atlantic and Pacific States was undeniable, he questioned the advisability of increasing at present our agricultural production.

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* This paper was subsequently separated into its two natural divisions, and the part relating to the "physical features" was published with illustrations in the "Popular Science Monthly" for September, 1884 (Vol. XXV. pp. 594–605), while that relating to the "economic features" appeared in "Science" for August 29, 1884 (Vol. IV. pp. 166–168).

249TH MEETING. MARCH 15, 1884.

The President in the Chair.

Fifty members present.

The Chair announced the election to membership of Messrs. MARK BRICKELL KERR, SAMUEL HAYS KAUFMANN, JOSEPH SILAS DILLER, CHARLES HENRY WHITE, and WILLIAM LAWRENCE.

Mr. G. K. GILBERT made a communication on

THE DIVERSION OF WATER COURSES BY THE ROTATION OF THE EARTH.

[Abstract.]

It being admitted that the rivers of the northern hemisphere are, by the rotation of the earth, pressed against their right banks, and those of the southern hemisphere against their left banks, it remains to determine whether this pressure is quantitatively sufficient to appreciably modify the courses of rivers. Opinion is divided, and the results of observation have been largely negative. Those who regard the cause as insufficient to produce observable results have approached the subject from two points of view, which are illustrated by the discussions of Messrs. Bertrand and Buff. The former computes that a river flowing in N. lat. 45° with a velocity of three metres per second exerts a pressure on its right bank of $\frac{1}{23500}$ of its weight, and regards this pressure as too small for consideration. The latter points out that the deflecting force, by combining with gravitation, gives the stream's surface a slight inclination toward the left bank, thereby increasing the depth of water near the right bank, and consequently increasing the velocity of the current at the right. This increment of velocity has a certain erosive effect, but it is regarded as less than that assignable to wind waves on the same water surface, so that the prevailing winds have a more important influence than the rotation of the earth.

The object of the paper is to consider the theoretical effect from a new point of view. The form of cross-section of a stream flowing in a straight channel depends on the loading and unloading of detritus, and is essentially stable, its character being naturally restored if accidentally or artificially modified. The distribution of velocities within this cross-section is symmetric, the swiftest threads of the current being in the center and the slowest adjacent to the banks. If now curvature be introduced in the course of the channel, centrifugal force is developed. This centrifugal force is measured by the square of the velocity, and is therefore much greater for the swift central threads of the current than for the slow lateral threads. The central threads, tending the more strongly toward the outer bank, displace the slower threads at that bank, and the symmetry of the distribution of velocities is thus destroyed. As pointed out by Thomson and others, this redistribution of velocities determines the erosion of the outer bank and the simultaneous deposition of detritus along the inner bank.

It has been shown by Ferrel that the deflecting power of the rotation of the earth upon a body moving on the surface is equivalent to the centrifugal force which would be developed if the body followed a circular course with radius of curvature (ρ) equal to $\frac{v}{2\,n\cos.\,\theta}$. In this expression v is the velocity of the body, n the angular velocity of the earth's rotation, and θ the polar distance of the locality.

The effect of rotation on a stream being equivalent to a centrifugal force is identical in kind with the effect of curvature of channel,* and this identity renders a quasi-quantitative comparison possible. Humphreys and Abbott found during flood a mean velocity of the Mississippi river at Columbus of 8.4 feet per second. The value of ρ corresponding to this velocity and the polar distance of the locality is about 20 miles. The actual bends of the channel in the same region, which depend for their features on the velocity and volume of the river at flood stage, have a radius of

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* The author has since seen reason to modify this statement. The two effects are not strictly identical in kind, because the effect of rotation varies with the first power of the velocity, while the effect of curvature of channel varies with the second power. For this reason the selective power of curvature is, for the same deflective force, double the selective power of rotation. The introduction of this consideration would modify the numerical results derived from the Mississippi river, but would not impair the qualitative conclusion. A modified treatment of the subject will be found in the American Journal of Science for June, 1884; Vol. XXVII, pp. 427–432.

curvature of about 1½ miles. Centrifugal force being a simple inverse function of radius of curvature, it follows that the deflective force by which the river is impelled toward its right bank by virtue of rotation is proportioned to the force by which it is impelled toward its outer bank on acute bends in the ratio of 1½ to 20. That is to say, in this particular instance the rotational deflective force is 7½ per cent. of the deflective force from curvature of channel.

The process of lateral corrasion is so complex that it is impossible to convert this result into terms of erosion and consequent deflection of stream channel, but a consideration of the manner in which the two deflective forces are combined sufficiently indicates that that due to rotation cannot be ignored. Wherever the stream bends toward the left the centrifugal force developed by the curvature is augmented by the rotational force; wherever the stream turns toward the right the centrifugal force is diminished by the amount of the rotational force; so that the tendency of the swiftest threads of current to approach the outer bank must be notably greater in one set of bends than in the other.

If this analysis of the subject is legitimate, the rotation of the earth ought surely to modify the courses of rivers to such extent that the modifications are observable phenomena. Exception should however be made of two important cases: first, rivers which are rapidly deepening their channels are by that fact held rigidly to their original courses, and are not deflected either by rotation or by any other cause; second, those parts of rivers whose function is deposition instead of erosion, should theoretically, under the influence of rotation, built their alluvial plains higher on the right hand side than on the left, and having established an inclination of the alluvial plain toward the left, should thereafter meander over the plain with equal facility in all directions. It is only in the middle courses of streams, where the work performed by the water is chiefly that of transportation, that the discovery of the effects of rotation should be expected.

Mr. WARD remarked that in the regions especially discussed the river courses are, in general, southerly, while the prevailing winds are westerly, so that the influence of the winds is opposed to whatever influence may be exerted by rotation. Mr. ABBE said that the tendency of driftwood toward certain river banks, cited by von Baer, had been plausibly explained as due to prevailing winds, but such action is purely or chiefly superficial, and a less important factor in erosion than the behavior of the main current, which is comparatively little influenced by winds. Nevertheless, he was surprised that the rotational influence admitted of so large a quantitative expression.

Mr. Dall said that the northward-flowing rivers entering the Arctic ocean afforded at their mouths no evidence of the effect of rotation. The summer winds of Arctic regions are from the northeast and east, and these produce on the north coast of America a shore-current, which drifts the beach sand and shingle westward, and deflects the river-mouths in the same direction. All the rivers from the Mackenzie to Point Barrow illustrate this tendency. On the coast of Siberia the fresh water discharged by the large rivers has been observed to turn eastward, although the winds would tend to throw it the opposite way. The Arctic ocean is there deeper; and it is believed that its principal currents are controlled by the northeasterly set of the general currents of the North Atlantic.

Mr. Robinson spoke of the indirect influence of wind on river channels, through drifting sand. Mr. Hazen pointed out that the influence of wind might be eliminated from the problem by studying the streams running east or west. Mr. Boutelle suggested that the course of the Mississippi did not indicate any result of rotational influence. Mr. E. Farquhar inquired whether the behavior of the Gulf Stream and other ocean currents was in accordance with the theory of rotational influence; and Mr. Dall responded that in the discussion of ocean currents this cause had lately dropped out of sight, the determination of courses being ascribed to the winds.

Mr. Mussey inquired whether the acuteness of continental masses toward the south admitted of an explanation based on the effect of terrestrial rotation; and Mr. Dutton responded by saying that the mass of speculation in regard to the recurrence of certain forms of continental outline had never really accomplished more than the statement of the fact. The fact itself is an accident, dependent on the volume of the ocean and the general laws governing the formation of mountain chains. If the ocean were five hundred feet deeper, or five hundred feet shallower, the forms of continents would be so far different that all the existing resemblances would disappear. The pointed extremities of some continents are merely expressions of the fact that mountain chains are more or less linear, and do not hold the same height throughout their whole extent.

Mr. G. E. CURTIS read a paper on

THE RELATIONS BETWEEN NORTHERS AND MAGNETIC DISTURBANCES AT HAVANA,

upon which remarks were made by Messrs. ABBE and COFFIN. [It will be published by the Army Signal Office as *Signal Service Note No. XIII.*]

Mr. GILBERT recurred to the subject of Mr. Russell's paper of the preceding meeting, and dissented from the view advanced by Mr. Antisell in regard to the origin of pumice. Mr. ANTISELL announced that he would discuss the matter more fully at some future meeting.

250TH MEETING. MARCH 29, 1884.

Vice-President MALLERY in the Chair.

Forty-two members present.

The Chair announced the election to membership of Messrs. BASIL NORRIS and WILLIAM STEBBINS BARNARD.

Mr. J. S. BILLINGS spoke briefly on

COMPOSITE PHOTOGRAPHY APPLIED TO CRANIOLOGY,

exhibiting several composite photographs of skulls. Adult male skulls of the same race were selected for composition and were photographed in sets of from 7 to 18—front, side, and back views being separately taken. The composition was directly from the skulls and not from the photographs.

Incidental mention was made of the uncertainty of measurements of cranial capacity by means of shot. Not only did different observers obtain widely different determinations from the same skull, but the same observer was not able to obtain closely approximate results in successive determinations.

Mr. G. Brown Goode made a communication on

FISHERIES EXHIBITIONS,

giving a list of all international exhibitions and describing especially those of Berlin (1880) and London (1883). The administrative systems of these two national exhibits were contrasted, and the social and economic results of the London exhibit were explained. [The substance of the paper will be published in the executive report on the London and Berlin exhibitions.]

Mr. M. H. Doolittle began a communication on

MUSIC AND THE CHEMICAL ELEMENTS,

but was unable to complete it before the hour for adjournment. The remaining portion was postponed until the next meeting.

By unanimous consent adjournment was deferred for a few minutes in order to afford Mr. Antisell an opportunity to reply to a criticism made at the previous meeting in regard to his views on the origin of pumice.

251st Meeting. April 12, 1884.

The President in the Chair.

Forty-one members and guests present.

Announcement was made of the election to membership of James Arran Maher, John Belknap Marcou, John Milton Gregory, Francis Tiffany Bowles, and William Eimbeck.

Mr. M. H. DOOLITTLE made a communication on

MUSIC AND THE CHEMICAL ELEMENTS.

[Abstract.]

The mathematical theory of music requires the satisfaction of the equation $2^x = \left(\frac{3}{2}\right)^y$ *nearly*; in which, for equal temperament, x = the number of equal intervals in the octave, and y = the number of these intervals that correspond to a nearly perfect fifth; and, for untempered music, x = the number of approximately equal intervals in the octave, and y = the number corresponding to a perfect fifth.

The above equation gives

$$\frac{x}{y} = \frac{\log \frac{3}{2}}{\log 2} \textit{ nearly} = \frac{176091}{301030} \textit{ nearly};$$

and by the method of continued fractions we obtain the succession of approximations $\frac{3}{5}$, $\frac{7}{12}$, $\frac{24}{41}$, $\frac{31}{53}$, &c.

For scales appropriate to major thirds, but disregarding fifths, we may substitute $\frac{5}{4}$ for $\frac{3}{2}$ in the above equations, and obtain the approximations $\frac{1}{3}$, $\frac{9}{28}$, $\frac{19}{59}$, &c. For the chord having the vibration ratio 7 : 4 (called by Ellis the subminor seventh), we may obtain in like manner the approximations $\frac{4}{5}$, $\frac{21}{26}$, &c.

Since $\frac{1}{3} = \frac{4}{12}$, the first two series of approximate fractions include a common scale of twelve intervals to the octave, of which seven intervals give the fifth, and four give the major third. The first and the third of these series include a scale of five intervals to the octave, of which three constitute the major third, and four constitute the subminor seventh. There is some reason to believe that this is the scale of Japanese music, with the intervals $\frac{7}{6}$, $\frac{8}{7}$, $\frac{9}{8}$, $\frac{7}{6}$, $\frac{8}{7}$. Five-tone scales have universally prevailed in early music; but it is questionable whether the vibration ratios have in any case involved the prime number seven. It would be interesting to know what scale best represents the songs of wild birds.

There is much reason to believe that simple mathematical principles underlie the phenomena of chemistry. It is not, *à priori*, absurd to suppose that matter in some way conforms to the properties of the primes 2, 3, and 5; in which case such derivative numbers might be expected prominently to appear as prominently occur in the science of music. The fraction $\frac{7}{12}$ might reasonably be expected.

If all the keys of a piano should be arranged seven consecutive keys in a line, the next seven in the next line, and so on, the columns give successions of fifths. It has been shown that if the chemical elements are arranged in the order of their atomic weights in lines of seven, the columns contain elements remarkably similar to each other. We seem to have a chemical scale remarkably analogous to the ordinary musical scale. If the piano keys be arranged in lines of twelve, the columns give octaves; but nothing is developed from a similar arrangement of the chemical elements, whence it may be inferred that the observed analogies are accidental, and have no true logical basis.

If the intervals of the chemical scale could be supposed to correspond to the seven intervals of the diatonic scale, the non-appearance of the twelve-fold relation would be accounted for; but, while the diatonic scale may have some claim to be called natural, it is not directly established by algebraic investigation of the relations of prime numbers. Until the discovery of chemical flats and sharps, there will be insufficient reason to regard the present chemical scale as diatonic.

Mr. LEFAVOUR illustrated the connection between tone and wave-length by means of a logarithmic spiral of base 2, the harmonic notes having *radii vectores* equal to multiples of the principal note.

Mr. ELLIOTT said he had learned from Mr. Poole that he had endeavored, in his euharmonic organ, to produce perfect chords in all keys without temperament.

Mr. KUMMELL remarked that in modern music the intervals of the major and minor thirds are the most important, because without them there is no harmony. This is also apparent from the well-known rule in thorough-bass that a third with its fundamental note is to be treated as a complete chord. Now it happens, in dividing the octave by equal temperament into 12 equal parts, that a major third is nearly 4 and the minor third nearly 3 of these, and thus we obtain not only tolerable fifths, but also tolerable thirds, and the requirement of thirds for harmony is approximately fulfilled. They are still better fulfilled, of course, if we divide the octave into 41 or 53 parts, as Mr. Doolittle has shown. As to the seventh harmonic, Poole and Helmholtz rightly hold that it should be and is used by instruments which can temper. It is obviously the fourth element of the chord of the dominant G, B, D, F, the F being the seventh harmonic to the G two octaves below (nearly so in equal temperament and exactly in natural harmony), and this chord in modern music forms the opposing harmony to the tonic chord C, E, G, in major, and C, E flat, G, in minor. Instruments with fixed tones like the piano-forte have to use equal temperament, and thus virtually reject all natural harmony except the octave. This defect is generally inappreciable in very slow movements, but may be noticed by a very cultivated ear.

Other remarks were made by Messrs. CLARKE, MUSSEY, and HARKNESS.

Mr. H. FARQUHAR read a

REVIEW OF THE THEORETICAL DISCUSSION IN PROF. P. G. TAIT'S "ENCYCLOPÆDIA BRITANNICA" ARTICLE ON MECHANICS.

[Abstract.]

This article covers seventy-four quarto pages in the last edition of the Encyclopædia, and gives a thorough mathematical treatment of the subject. No innovations calling for comment—unless an extended use of the "fluxional" notation for derivative functions be so regarded—appear until near the end, where two and a half pages are devoted to a disproof of the objective reality of force, and an advocacy of the disuse of the term in scientific writing. The character of the publication, and the eminence of the author in mathematics and physics, entitle his arguments to a careful examination.

In the first place, Prof. Tait infers that force can have no such reality as matter has, because it is to be reckoned positively and negatively—an action being opposed by reaction—while matter or mass is signless. This suggests two comments: (1), the author never questions the objective reality of space and time, of which realities it is an essential feature that to every direction or interval A–B, an equal direction or interval B–A, of opposite sign, corresponds; (2), the idea of a negative mass is not a self-contradictory one, and was once generally accepted. The element phlogiston was given up not because of any absurdity in ascribing levity to material substance, but because a form of matter with positive mass (oxygen), capable of explaining all the phenomena, had been actually separated and identified.

Prof. Tait's next criterion of objective reality is quantitative indestructibility, an attribute shared by time, space and matter, to which he adds energy. But the evidence of the indestructibility of energy is not of the same nature as that of the indestructibility of matter; for the latter in all its forms may be localized, and its density or elasticity measured; while the former, when stored up or "potential," cannot be shown to possess any of the properties of energy kinetic, or any existence in space, or any objective character whatever. Prof. Tait admits this difficulty virtually, and awaits for its solution the discovery of some evidence "as yet unexplained, or rather unimagined." All strains and other actions of a clock-weight on its supports are obviously precisely the same—or impalpably somewhat stronger—with the weight wound up an inch, as with it wound up a yard; and the existence of a greater "potential energy" in the latter case is to be found not in the clock, but in the mind, which requires this expression as a form in which to put its conviction that a certain greater amount of work can be obtained. Even though it be admitted that there are no other intelligible terms in which this conviction can be stated, it is clear that the indestructibility of energy is an ideal and subjective truth, and cannot, therefore, be relied on as evidence of a reality distinctively "objective."

A third point made by Prof. Tait against force is that its numerical expression is that of two ratios: "the space-rate of the transformation of energy" and "the time-rate of the generation of momentum." These results are obtained by simple division, in an equation which expresses the fact that the work done by a body in falling the distance *h* is just that required to lift it through *h* against gravity. The fallacy involved in treating the numerical expression for force as force itself, has been well exposed by Mr. W. R. Browne (in a criticism of the same article, *L. E. D. Phil. Mag.* for November, 1883); and the assumption that ratios are necessarily non-existent is even more fallacious. Were it trustworthy, Prof. Tait's deductions would not be the only ones admissible. His equations would lead quite as conclusively to proofs of the non-objectivity of space and time (the former becoming the rate of work-units, the latter of motion-units, per unit of force), and so to a confirmation of the celebrated German view, that that which is universal and necessary in thought, belongs to the Subject; or they might even give mass in the form of a ratio, and hence suggest the non-objectivity of matter.

Not the least of the Professor's objections against force, it would appear, is that it is "sense-suggested." It is a mere truism to say that no other suggestor is possible, within the domain of science. It is, perhaps, better worth while to call attention to the indubitable fact that the real, if not the avowed, ground of the objection against "action at a distance," entertained by many physicists, is that such action is not directly suggested by sense-impressions. This is what they must mean by calling it "occult;" actions as our consciousness knows them, and as we can produce them, being generally characterized by proximity undistinguishable from actual contact. Further, if there is any reproach in this epithet, energy is quite as open to it as any function of energy can be. In fact, our senses directly report work, in the form of nerve-disturbance, and nothing else. Force is no more truly an inference from nerve-reports testifying of energy exerted, than is matter. In fact, the inference of the independent existence of matter is the less direct and more questionable of the two. The view advocated by Mr. Browne, following Boscovich, that matter is but "an assemblage of central forces, which vary with distance and not with time" or with direction, is one of great simplicity as well as suitability to analytic treatment, and one of which no disproof is possible.

The paper was discussed by Messrs. DOOLITTLE and ELLIOTT.

252D MEETING. APRIL 26, 1884.

Mr. HARKNESS in the Chair.

Thirty-eight members and guests present.

Announcement was made of the election to membership of Messrs. DAVID PORTER HEAP and THOMAS MAYHEW WOODRUFF.

Mr. J. R. EASTMAN made a communication on

A NEW METEORITE.

[Abstract.]

A mass of meteoric iron weighing 113 pounds was acceccidently discovered in the making of an excavation at Grand Rapids, Michigan, and was examined by the speaker in 1883. One face shows evidence of fracture, and the greater part of the remaining surface, of fusion. A very small sample submitted to Mr. F. W. Taylor for chemical examination had a specific gravity of 7.53 and a composition:

Iron	94.54
Nickel	3.81
Cobalt	.40
Insoluble (about) . .	.12

The stone is supposed by its holders to consist of gold and silver, and to be the buried treasure of a miser. This delusion has caused it to form the subject of a lawsuit.

The communication was discussed by Messrs. BATES and F. W. CLARKE.

Mr. W. H. DALL read a paper on

CERTAIN APPENDAGES OF THE MOLLUSCA.*

* Published in the American Naturalist, Vol. XVIII, pp. 776–778.

Mr. J. S. DILLER made a communication on

THE VOLCANIC SAND WHICH FELL AT UNALASHKA OCTOBER 20, 1883, AND SOME CONSIDERATIONS CONCERNING ITS COMPOSITION.

[Abstract.]

The sand is composed chiefly of crystal fragments of feldspar, augite, hornblende, and magnetite, with a considerable proportion of microlitic groundmass and a very few splinters of volcanic glass. Its mineralogical composition is that of a hornblende andesite; but the chemical analysis by Mr. Chatard shows it to contain only 52.48 per cent. of silica,—which is much more basic than the average for that group. The character of the minerals, as well as the general composition of the sand, indicated so clearly that the crater from which it must have issued was erupting hornblende-andesite, that I was led to seek an explanation for its paucity in silica.

With this purpose in view, a number of volcanic sands and dusts from various parts of the world were examined and compared with the lavas to which they belong. First and most important among these is a sand from Shastina, a crater named by Captain Dutton, upon the northwestern flank of Mt. Shasta, in northern California. This sand, like that from Unalashka, is composed chiefly of crystal fragments of feldspar, augite, hornblende, and magnetite, with fragments of microlitic groundmass. Besides these, there are many pieces of hypersthene crystals and pumiceous glass. The sand contains 60.92 per cent. of silica, while the hornblende-andesite lava (rich in hypersthene) of Shastina, to which the sand belongs, contains 64.10 per cent. of silica.

From these and other examples it may be stated as generally true that volcanic sand is composed essentially of crystalline fragments, and contains less silica than the lava to which it belongs.

With volcanic dust, however, the case is different. Microscopical examination shows that it is composed chiefly of volcanic glass particles; and as far as chemical analyses have been made, they indicate that volcanic dust is more silicious than the lava to which it belongs.

That volcanic sand should be crystalline and basic, and the accompanying dust vitreous and acidic, as compared with the lava

to which they belong, is not merely determined by accidental circumstances, but has its inception in the magma before the eruption takes place. By the process of crystallization magmas are frequently divided into a crystalline solid portion, and an amorphous more or less fluent portion. Basic minerals are the first to crystallize, so that as the process advances the amorphous remnant of the magma becomes more and more silicious. The crystals are generally thoroughly intermingled with the amorphous magma, and in the latter are accumulated nearly all of the absorbed gases under great tension, so that when the pressure is relieved it may be blown to fine silicious dust, which may be carried by the wind many miles from its source, while the solid crystalline portion will contribute chiefly to the formation of sand, and be precipitated comparatively near the crater from which it issued.

In cases where no previous crystallization has taken place in the magma before it comes to violent eruption, the volcanic dust then formed will have about the same chemical composition as the lava to which it belongs. Mr. Russell has recently described an interesting case of this kind in the western part of the Great Basin. It appears to be generally true that if other conditions are favorable the difference in chemical composition between volcanic sand and dust is directly proportional to the amount of crystallization in the magma before its ejection.

The basic character of the Unalashka sand may be explained by supposing that the silicious portion of the magma was carried away in the form of dust.

The source of this sand is supposed by the collector, Mr. Applegate, the Signal Service Observer at Unalashka, to have been the new crater formed last autumn, near the Island of Bogosloff, about sixty miles away.

Mr. DUTTON spoke in commendation and amplification of Mr. Diller's contribution to geologic philosophy. Mr. DALL described the geographic relations of the volcano from which the Unalashkan dust was presumably derived, showing the improbability of the eruption having been directly observed. He spoke also of the distribution of the Aleutian volcanoes and the lithologic characters of their *ejectamenta*.

There ensued a general discussion of the nature and properties of volcanic dust and of the theory which ascribes recent meteorologic phenomena to the dust ejected by Krakatoa. In this Messrs. DUTTON, PAUL, W. B. TAYLOR, DILLER, ROBINSON, and WARD participated. Mr. DUTTON pointed out that their process of formation tends to give volcanic dust particles a quasi-definite size, and probably does not produce a large amount of dust fine enough for indefinite suspension. The greatest distance to which volcanic dust has been definitely ascertained to travel is eight hundred miles.

Mr. PAUL argued from the violence of the Krakatoan explosion its competence to charge the atmosphere at very great altitudes, and considered the fineness of the dust a sufficient explanation of its indefinite suspension.

Mr. TAYLOR said the phenomenon to be accounted for was specially remarkable, first, for the unusual elevation of the finely-divided smoke or dust extending far above the highest cirrus clouds, or probably to twenty or thirty miles above the earth's surface (as shown by its twilight duration); secondly, for its wide diffusion (covering a large fraction of the terrestrial atmosphere); and thirdly, for the long continuance of its suspension in the air (extending over many months). Mr. Lockyer and Mr. Preece had suggested an electrical condition of the matter as favoring both its extraordinary diffusion and its equally extraordinary suspension. This hypothesis seemed to the speaker very plausible. Electricity is a phenomenon of volcanic eruption, and dust particles charged with electricity in the same sense with the earth would be repelled not only by one another, but by the earth. At thirty miles above the ground the air is not only very rare, but is practically anhydrous, and the discharge of electricity is impossible.

Mr. DILLER, in response to a question by Mr. Paul, said that the microscope reveals no limit to the fineness of Krakatoan dust. The higher the magnifying power applied, the greater the number of particles visible; and this relation extends to the limits afforded by the capacity of the instrument. To more powerful microscopes, yet finer particles would presumably be visible.

253D MEETING. MAY 10, 1884.

The President in the Chair.

Fifty-four members and guests present.

Announcement was made of the election to membership of Messrs. JOHN MURDOCH, ROMYN HITCHCOCK, WILLIAM SMITH YEATES, GEORGE PERKINS MERRILL, and FREDERIC PERKINS DEWEY.

It was announced that a vacancy in the General Committee, occasioned by the resignation of Mr. J. J. KNOX, had been filled by the election of Mr. F. W. CLARKE.

By invitation, Mr. G. H. WILLIAMS, of Baltimore, Maryland, addressed the Society on

THE METHODS OF MODERN PETROGRAPHY,

first, defining the field of petrography, and second, discussing the methods of petrographic investigation. These methods are: (1), chemical; (2), mechanical; (3), optical; (4), thermal. The chemical methods are quantitative and qualitative. The mechanical methods include the separation of the constituent minerals of rocks by precipitation in heavy solutions and by the use of electro-magnets. The optical methods include the preparation of thin sections, their examination by transmitted ordinary light, and their examination by polarized light, for the determination of crystallographic system, pleochroism, and angles of extinction. The thermal methods are chiefly synthetic, consisting in the artificial production of mineral aggregates for the purpose of determining the processes of their natural production. By the regulation of temperatures in fusion and refrigeration all varieties and all structures of basic rocks are reproduced. Acidic rocks have not been thus reproduced, and it is believed that great pressure is a condition of their genesis.

Mr. DUTTON spoke of the bearing of modern petrographic investigations on some of the greater problems of geology.

There followed a symposium on the question

WHAT IS A GLACIER?

[Abstract.]

Mr. I. C. RUSSELL: In framing a definition of a glacier it is evident that we must include both alpine and continental types, and also take account of the secondary phenomena that are commonly present. With this preamble we may define a glacier as an ice-body, originating from the consolidation of snow in regions where the secular accumulation exceeds the loss by melting and evaporation, *i. e.*, above the snow-line, and flowing to regions where loss exceeds supply, *i. e.*, below the snow-line.

Accompanying these primary conditions, many secondary phenomena dependent upon environment, as crevasses, moraines, lamination, dirt-bands, glacier-tables, ice-pyramids, etc., may or may not be present.

Mr. S. F. EMMONS: The glacier is a river of ice, possessed, like the aqueous river, of movement and of plasticity. In virtue of the latter quality it adapts itself, though more slowly, to the form of the bed in which it flows. The névé field is the reservoir, from which it derives not only its supply of ice, but the impulse which gives it its first movement. The névé is formed by the snows which accumulate in relatively wide basins above the snow-line from year to year, living through the heat of summer. Its mass may be more or less compact, according as it is thicker or thinner, and it may have a certain movement, which will be greater or less, according to the greater or less inclination of the basin; but until it moves from its wide and shallow bed into a narrower and deeper one, and thus gives outward proof of the plasticity of the ice of which it is composed, it does not become a glacier. It may be crevassed. Often a long crevasse at its upper edge gives definite proof of its movement; and this movement may cause a cracking or crevassing in other points, resulting from the unevenness of its bed. It may or may not carry blocks of rock on its surface, but these would be rare, and never in the well-defined moraine ridges that are formed upon the glacier proper. Not, however, until its form had essentially changed to fit the bed in which it flows should it be considered to constitute a glacier proper.

Mr. W J McGee: The phenomena of glacier ice and névé ice appear to belong to a graduating series; and in consequence the two phases can only be arbitrarily discriminated. Any classification depending upon coincidence of the *loci* of apparent transition from the first phase to the second with *loci* of sudden constriction or abrupt acclivity in the valley is artificial and incompetent, since such coincidence is fortuitous; the classification depending upon the ability of the second phase to sustain bowlders upon its surface is superficial and incompetent (provided such ability be due to density of the ice), since the sub-surface density of the névé, being determined by its age and the pressure of the superincumbent mass, must, in some portions, equal the surface density of the glacier; and the classification depending upon rate of motion is equally incompetent, since motion is common to the entire ice-mass, and abruptly varies only where conditions of glacier-bed are suddenly variant. Arbitrary diagnostic characters may and should be, however, agreed upon by consense among glacialists. Perhaps the most satisfactory line of demarkation detectable is the *snow-line*, above which superficial débris is buried by precipitation, and below which it is exposed by ablation.

Mr. W. H. Dall: It is proper to discriminate masses of ice moving in a definite direction from the immense fields of ice which are practically stationary. The term "glacier" should be restricted to the former. A glacier is a mass of ice with definite lateral limits, with motion in a definite direction, and originating from the compacting of snow by pressure. Moraines are not diagnostic; and the definition should not include those masses of arctic ice which, by reason of their low temperature, are fixed in position.

Mr. T. C. Chamberlin: Nomenclature is a matter of convenience. When subjects rise into familiar thought and frequent reference brevity of expression calls for specific names. But terms arising thus from a natural demand are not closely discriminative. Hard and fast lines of demarcation do not prevail in nature, but rather gradations of character. Were it otherwise names of sharply-defined application could be more freely used. The terms *névé* and *glacier* doubtless originated to satisfy the convenience of guides and travelers, and were without strict scientific application. In attempting to give them scientific definition, I think we shall fail of satisfaction by making them structural terms. The better distinction is genetic. There is an area of growth and an area of waste to every glacier, and the distinct recognition of the two in quaternary glaciers is likely to rise to some importance. Superficially the area of growth coincides with the névé; the area of waste is that of the glacier proper. From every annual snow-fall there remains, at the time of maximum summer melting, a remnant that feeds the glacier. This is the névé for that year. The area may be greater or less in different seasons. The névé-field is accurately shown only on the day of maximum waste.

A contribution of much value, bearing upon the property of ice which permits glacier motion, has recently been made by Petterson, who has demonstrated, by refined experimentation, that ice, especially if impure, *shrinks* as it approaches the melting point and becomes plastic.

Mr. C. E. Dutton desired to reiterate the remarks of Mr. Chamberlin to the effect that definitions can rarely or never be made rigorous. Glaciers, no doubt, vary in their characteristics like almost all other groups of phenomena. There is little difficulty in recognizing a glacier when all those features which characterize it are present, and when the conditions are of the ordinary nature. But exceptional cases arise. The lower parts are sometimes wanting and the névé alone remains, or the portion where the névé passes into the glacial stream may constitute the termination. In the latter case those who desire to be extremely precise in their phraseology might hesitate. It should seem best, whenever an occurrence is modified or defective, to use the term "glacier," with a qualification which shall express the particular circumstance.

Remarks were also made by Messrs. Gilbert and Elliott.

254th Meeting. May 24, 1884.

The President in the Chair.

Twenty-six members and guests present.

It was announced from the General Committee that after the 255th meeting, June 7, the Society would take a vacation until October 11.

A request on behalf of the coming Electrical Exhibition at Philadelphia for instruments and books was communicated to the Society.

Mr. H. H. BATES read the following paper on

THE PHYSICAL BASIS OF PHENOMENA.

If there is anything entirely disheartening, it is to see the few landmarks of human achievement disappear before the shifting current of opinion, as headlands disappear under the ceaseless buffeting of the ocean. It is no doubt a matter of poignant regret to the cherisher of ardent theological convictions to see the bulwarks of faith slowly undermined by controversy. So, also, to him who has built his convictions on supposed demonstrable and irrefragable fact, to find nothing unassailable, not even the axioms and postulates conceded for ages as first principles, on which the fabric of science was reared, nor the sublime inductions of Galileo and Newton, on which the modern philosophy called natural—the only fruitful philosophy which man has produced—has been founded.

But the course of criticism shows that there are no first principles. Nothing is unquestionable. Even the mathematic joins hands with the metaphysic. I propose briefly to examine the fundamental grounds of mechanical philosophy, in view of the wide divergence of basal hypotheses in recent years, and especially on account of the importance conferred upon certain speculations by their admission into works of standard reference and authority.*

To do this aright it is necessary to go behind the mere sub-science of mechanics to the essence and substance of things, as did the eighteenth-century philosophers succeeding Newton. The observational data which have accumulated since that time by the splendid efforts of the molecular physicists enable us to review and recast, with some promise, the primary dogmas regarding the physical basis of phenomena. It is legitimate to frame hypotheses on subjects which are still unfathomed, but which confessedly do not belong to the domain of the unknowable. The distinguished example of the authors of the vortex atom would alone justify such a conclusion.

No entirely satisfactory hypothesis of the atom has yet been

* Encyclopædia Britannica, 9th Ed., Articles "Mechanics," "Measurement," etc.

found. I do not design to discuss the vortex atom here at length; for, although it is the most successful form of the Cartesian doctrine of vortical substance, it has not been perfected, and is generally regarded rather as an example of remarkable speculative and mathematical ingenuity, than as a discovery, corresponding with any facts of objective physics. It has insuperable difficulties, some of which have been pointed out by Clifford, and others by Clerk-Maxwell. Moreover, unparticled or continuous substance, the necessary postulate in this hypothesis, is something we not only have no experience of, but find full of inconsistencies with experience, when we gain a clear conception of what it implies. Such a conception fulfills Hegel's paradox that being and non-being are the same, since it forbids all mobility, all differentiation, as was perceived by the followers of Democritus. It simply affords an inviting basis for analytical discussion, on account of the elimination of the very conditions of objective existence which make the mathematical difficulty.

There are some postulates regarding substance which we may probably be permitted to assume at the outset. We may postulate its objectivity, and also its discontinuity. I have no space to review here the time-worn controversy between continuous and discontinuous substance. The arguments, which are exhaustive from the metaphysical side, are as old at least as Democritus and Anaxagoras. Suffice it to say that modern experiential philosophy has decided the battle experimentally in favor of the discontinuity of matter. The dispute only lingers in the region of the atom, where observation cannot penetrate or has not penetrated. The inability to conceive which attaches to all non-experiential affairs is encountered here, coupled with the too great facility of conceiving what is superficially observed, but will not bear analysis. Thus our first impressions of substance are in favor of its continuity. It is only after much reflection that we get the idea of necessary discontinuity, as bound up with the exhibition of existing phenomena. But the wonderful development of the Cartesian mathematics, in conjunction with the infinitesimal calculus, and its great facility in dealing with geometrical continuities, has tacitly revived the Cartesian idea regarding the nature of matter, as synonymous with space relations, which never reached intelligible development at the hands of its author, and wholly declined and disappeared after the establishment of the Newtonian philosophy, and the discovery of the discrete character of substance.

In point of fact, experience would point to extreme porosity or discreteness as characteristic of substance, rather than to its opposite—perfect continuity. The infinite divisibility of space has nothing in the world to do with the question, though this is a confusion often fallen into. On the contrary, there is an infinite distinction between the infinitesimal discrete units of substance, occupying extension by their interactivity, and the passive infinitesimal resolvability of space continuity. This is the antipodean difference between the Epicurean and the Cartesian conceptions; the former admitting of the operations of force, the free exhibition of motion, the organization of material phenomena, which are phenomena of mobility; the latter constituting a plenum, with only ideal divisions, and phenomenally as necessarily barren a negation as space itself.

Substance is purely experiential. In its essence it is still incomprehensible, because experience has not yet reached down to those recesses. We know nothing of substance except by its manifestations. These manifestations are cognized by us through sense impressions, weighed, compared, adjusted, and analyzed in the mysterious alembic of the mind. First impressions have enormous predominance, and are intensified by heredity of cerebral predisposition and function.

We cognize substance only in bulk by direct perception, and these vast aggregations stand in thought for matter. A drop of water contains incomparably more molecules than the ocean contains drops; a grain of sand more particles than the earth contains grains; and it is this vast mesh of complicated forces that forms the integrated concept of matter to our apprehension. The child, before he can walk, encounters obstacles to movement, reaction to his every muscular effort, of equal measure to his own; and thus his first and profoundest convictions of objective existence are associated with resistance, opposition, repulsion. This impression of matter is so early that it remains with us as its most natural and obvious characteristic.

The idea of weight is also one of the earliest experiences. This idea would not be conceivable to a denizen of the deep sea, for our first ancestor who emerged from the water gained the experience at the cost of great struggle and enterprise. By the natural development of muscle and function the child rears itself very early against the constant pull of our pedestal, triumphs over it with new-found energies, dances on tiptoe, and spurns the ground, but is soon content to draw the battle, to wander around a few weary years on equal terms, at length to call in the aid of a stick or crutch, and, finally, to resign the unequal contest, and sink, vanquished and satisfied, to rest in its bosom. Weight thus seemed a natural characteristic of matter until identified and generalized by Newton as a universal and especially a reciprocal property. This generalization transferred the property, in conception, from the naturally heavy body to a cause outside thereof, namely, the earth itself. Here the human mind relucted, for, unlike repulsion, attraction is not an observational fact. All forms of tension, stress, constraint—by whatever name called—are attended in the child's experience with an intermediary connection. The string is necessary to pull the cart, and the action of the magnet upon the iron particles is viewed with astonishment and awe. The sense of mystery does not proceed so far in his case as to contemplate the equally mysterious power which makes his string differ from a rope of sand. The most profound attention of the human mind has not yet fathomed this mystery.

Inertia or mass is a less obvious property, being in early observation and in common apprehension bound up with weight. It was not recognized in philosophy till Galileo's time, nor is it now by the common perception, except after training. A lady makes no scruple of asking to have a loaded car or train or vessel stopped at a given point on the instant, and reinvested with motion any number of times; and would-be inventors often contrive theoretical machines having numerous heavy reciprocating parts timed to velocities impossible of execution. With beings under other conditions it is wholly different. The sword-fish, *e. g.*, can have no conception of gravity, as he has no perception of it, but his apprehension of inertia is finely cultivated, through the muscular sense, in setting up and modifying the rapid movements in which his existence delights, as well as through his vivid realization of momentum, in the piercing of a whale or a vessel, by which his function is so powerfully exhibited. When once realized by human perception, however, inertia becomes identified with substance as its most primary characteristic.

The old scholastic property of impenetrability, also, is one of the superficial notions of experience, gained in the same way as that of repulsion. It seems to pertain to solids—the typical matter—with approximate accuracy, though calcined plaster of Paris and water, *e. g.*, will occupy a good share of each other's volume, and still form a highly porous solid. But a quart receiver full of hydrogen can have a quart of carbonic acid gas deftly introduced into it as into a void space; and so can a quart of water, at ordinary temperature and pressure, according to Gmelin, without increase of volume, although water is the type of material continuity. As to impenetrability in the molecule we can predicate nothing. The evolution of heat in chemical combinations indicates penetration of volume, with reorganization of the molecule in less space; and there is no reason, except a scholastic one, why two or more molecules, or even atoms, should not occupy the same place, as admitted by the highest authority—James Clerk-Maxwell.

Dimension is also a common notion, derived similarly from supeficial and early experience. Solids alone have figure and assignable dimension, though liquids have fixed volume, and gases variable volume, in inverse ratio to constraint; but even solids are of varying and fluctuating dimensions, according to temperature, density, etc. Solidity and liquidity are, it is well known, but mere transitory conditions of material aggregation, for all matter is capable, by sufficient accession of molecular motion, of assuming that hyperbolic or expansive condition which we call gaseous, and in this state dimension and impenetrability are meaningless terms. Concerning dimension as a necessary attribute of the unit of mass, Clerk-Maxwell says (Encyclopædia Britannica, 9th Ed., Vol. 3, p. 37): "Many persons cannot get rid of the opinion that all matter is extended in length, breadth, and depth. This is a prejudice * * * arising from our experience of bodies consisting of immense multitudes of atoms." That there is no necessary relation between mass and volume as there is, *e. g.*, between mass and weight is shown to common experience by the notably different masses of a buck-shot and a pith-ball of the same dimensions, or of a cannon-ball and a child's hydrogen balloon. A pellet of iridium equivalent in mass to the pith-ball might be microscopic, and, by extreme supposition, infinitesimal. We are not forced, however, to deny to the unit of mass finite magnitude, as this would be an experiential fact when ascertained.

The remaining so-called properties of matter are too obviously transitory, accidental, or derivative to require attention. Color, luminosity, opacity, transparency, sapidity, sonority, odor, texture, temperature, diathermancy, plasticity, hardness, brittleness, density, compressibility, conductivity, malleability, fusibility, solubility, and many others, are too clearly but conditions of aggregation, or else mere subjective states due to the way the complicated interactions of the primary qualities affect our senses. What are the primary qualities?

Here is where the modern method of philosophy flags, by the disappearance one by one of the experimental means of approach, as we eliminate the non-essentials. But though the substance is thus elusory, we cannot yet believe it to be illusory.

Chemical and molecular physics have already gone marvellously beyond the ordinary range of sense-perception, by strictly scientific methods. Not only is the discrete character of matter established, but many data of the differentia and organization of the molecule are discovered. Here is a vast field of science in itself. From the ideal molecule, or simple couple, up through the 70 actual organized molecules of our provisional elements, then the chemical molecules of their combinations in vast numbers, discovered and undiscovered, and, lastly, the enormously complex organic molecule in infinite variety, the domain transcends in area for classification that of biologic science. The simple molecule has not yet been discovered, much less the molecular constituent, the atom, or the *indivisible*. It is evident, however, that the properties of matter which are essential, not differential, must reside in the atom. The philosophers succeeding Newton treated the atom and the elementary molecule as one, from lack of sufficient chemical knowledge. We are on a higher plane of information, but their method is not necessarily vitiated by such lack of distinction.

We cannot, as before said, attribute *à priori* to the atom dimension or figure, though we postulate it to aid conception. As the atom is an absolute unit, there is incongruity in finally assigning to it such relative attributes, which are but matters of comparison and degree. There are properties, however, which are inseparable from an absolute essence. These are the properties by which the essence is manifested to us. We know them provisionally as forces, in the Newtonian nomenclature. Had gaseous matter neither weight nor mass, we could not know of its existence. But these attributes are so constant in matter that we estimate its quantity in terms of them and have no other exact terms. Weight is the statical measure; mass the dynamical measure. And since weight and mass correspond for all substances, under all transformations, we judge that the correspondence identifies them alike with the essence. They cannot be the mere result of organization. They must belong to the ultimate atom.

At this point it would seem proper to attend to a question of definition. Definitions are essential to clearness, on the one hand, and a source of entanglement on the other, if we fall into the scholastic error of regarding a mere word as the coextensive symbol of an idea. Words are evolved during the imperfection of ideas, and language is still a most imperfect medium of expression. Hence, logic is not a science in the sense that mathematics is. I have used the term force. This is a word of much ambiguity of meaning. We may use it as a convenient mathematical expression for a mere rate of change of momentum, or we may go farther and *define* it, as that which changes a body's state of rest or of uniform motion in a straight line; either of which uses restricts it to only a portion of phenomena, and ignores the whole science of statics, dealing with forces in equilibrium and the phenomena of balanced stress. If we give it a more general signification, as that which changes or tends to change, or conserve, the state of motion of particles, or systems of such, either in quantity or direction, we embrace statics as well as kinematics, and get a measurably philosophical definition, if we bear in mind the proviso that we do not thereby postulate force as an entity apart from substance.

And since the compound variable space and time condition which we call motion (of which rest is but a phase) is the sensible resultant of the interaction of such discrete substance by constant rearrangement where readjustment is free, or the potential resultant where confined, we may admit that the observed tension and persistence, of whatever form, is that which effects the phenomenon (though masked by infinite variety and composition), and always across the discontinuity: not as separate entities, but as modes of manifestation of the interacting and pervasive substance itself and its only manifestations. This we call *force*—the inscrutable agent of phenomena—and this I take to be the true Newtonian conception, as evinced by his maturest conclusions, expressed in query 31 appended to his Optics. (B. 3, 2d Ed., 1717.)

So far as weight goes, it was generalized by Newton to be a reciprocal force or stress, operative without limit on the law which inheres in radial space relations—the inverse square of the distance. The term operative means effective upon mass, namely, bridging the discontinuity. Gravity is the typical attractive force—*vis centripeta.* The relation is mutual by the law of action and reaction, and amounts to a universal tension among particles, controlling all matter everywhere into orderly movements and relations. This is what we postulate from observation, on the Newtonian plan of naming simply what we see. The notion, however, of action at a distance has encountered a metaphysical difficulty in many minds, from the preconception derived from ordinary experience that all affections or stresses must proceed through an intermediary connection, deemed continuous. Even Newton made concession to this prejudice in his oft-quoted letter to Bentley. That there is really no such continuity in any mode of connection known is demonstrable, and the notion itself that the fancied continuity of some rare effluvium could in any way aid the mechanics of the problem is chimerical. Clerk-Maxwell, moreover, has shown (Nature, Vol. 7, p. 324; Encyclopædia Britannica, Vol. 3, p. 63) that action at a distance is as necessarily implied in repulsion as in attraction, so that theories of repulsion do not aid conception. Ability or inability to conceive, furthermore, is not held even by the metaphysicians to be a criterion of objective truth. Such truths exist independent of the conceiving mind. The conceiving organ was evolved by experience, and conception develops with attention. The first law of motion was wholly inconceivable to the contemporaries of Galileo, and we find such instances even now. Thus, while plain truths are inconceivable until established, some utter absurdities have been deemed conceivable, as, for instance, vacuity of two dimensions. State of mind, then, is no measure of external truth.*

* In this connection, to illustrate how entirely a matter of opinion or prejudice or culture is this notion of conceivability, I quote from a letter

The second force or manifestation of the atom, inertia,—or mass,—unlike gravity, is not unlimited in range of action. As to this property matter is discrete. Mass has both a *locus* and a limit (being apparently dependent for dimension on multiplicity), and amounts to that incomprehensible property by which conservation of motion is maintained. Under gravity, quantity of motion varies according to relations of contiguity, but under inertia motion is conserved in direction and quantity, is modified in direction and quantity by interaction of mass with gravity, and is redistributed by interaction with repulsive force upon an indefinitely near approach of particles, upon conservative principles. Its discreteness gives matter its numerical and finite character, and admits of that interplay which constitutes phenomena.* Its reality and primary

written by Faraday to Dr. Playfair, in response to some inquiries of the latter about his atomic opinions:

* * * "I believe in matter and its atoms as freely as most people—at least, I think so. As to the little solid particles which are by some supposed to exist independent of the forces of matter, and which in different substances are imagined to have different amounts of these forces associated with or conferred upon them, * * * as I cannot form any idea of them apart from the forces, so I neither admit nor deny them. They do not afford me the least help in my endeavor to form an idea of a particle of matter. On the contrary, they greatly embarrass me; for, after taking an account of all the properties of matter, and allowing in my consideration for them, then these nuclei remain on the mind, and I cannot tell what to do with them. The notion of a solid nucleus without properties is a natural figure or stepping-stone to the mind at its first entrance on the consideration of natural phenomena; but when it has become instructed, the like notion of a solid nucleus apart from the repulsion, which gives our only notion of solidity, or the gravity, which gives our notion of weight, is to me too difficult for comprehension; and so the notion becomes to me hypothetical, and, what is more, a very clumsy hypothesis." (Playfair's works, Vol. 4, p. 84.)

Here we see a difficulty opposite to that usually encountered, for, while many people profess an infirmity of conception of the forces apart from the imaginary vehicle, Faraday finds the vehicle of no use as a carrier of the properties, but a positive impediment.

* This property has a multiplicity of names in the Newtonian nomenclature, according to the varying aspect of its function. Thus, in the aspect of persistence of mass in state of rest or of motion uniform in direction

character, when once apprehended, have proved more acceptable to the imagination than has the conception of central force, and under appulsion hypotheses (with the aid of that other readily accepted property, repulsion, and certain highly artificial hypothetical media), it has been made to do duty in providing so-called explanations of gravity, under its form of *vis viva.*

It has always seemed to me that the mode of approach adopted by Boscovich was the most philosophical and rigorous of any. He viewed matter for the purposes of mathematical treatment and for investigation of its essentials, as divested of accidental and fugitive properties; and as the analytical calculus had not then become so developed as to wholly fascinate the attention of geometers with abstract and ideal relations, he proceeded from prime physical data. He thus identified matter by those apparently general and characteristic properties recognized by Newton as the basis of mechanical philosophy in conjunction with the laws of motion. These properties are, as before said, gravity, inertia, and repulsion; or, as characterized by function, attraction, conservation, distribution. In this view matter consists of certain *loci* of central forces, mutually attractive by the first property according to a variable law in the duplicate inverse ratio of distance without limit, but restricted in manifestation as to the second property to the infinitesimal *locus*, thereby excluding unitary dimension. Contemplating matter under this aspect alone, a dilemma arose. For gravity waxing by the law of inverse squares of the distance up to the focus or origin involves the consideration of infinite force and apparently of infinite velocity in the limit, in the supposable case of rectilinear ap-

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and quantity, *i. e.*, of resistance to change of state except in conformity with motion impressed, the property is called *vis insita*, which may be *vis insita activa* (momentum), or *vis insita passiva* (*vis inertiæ* of mass.) In its aspect of acquirement of a new state of motion by interaction with other forces or masses, Newton called the new state thus superposed *vis impressa*; which, when the operation of acquirement has ceased, becomes again *vis insita*. In its aspect of persistence of mass towards uniform direction of motion under the constant deflective stress of vector central force, it is called *vis centrifuga*. And in its active form, conditioned by motion acquired, its capacity for furnishing motion from its store, either for impressing motion upon other mass, with consequent loss, or for supplying the potential fund under the drain of adverse central force, is called *vis viva* (energy.)

proach, at which point the equations become unexplainable. While Euler and La Place differ in their interpretations of the result, Boscovich sought to solve the apparent absurdity and inconceivability by the invention of his ingenious and complex system of alternate spheres of attraction and repulsion, or change of sign, on a very near approach, with infinite repulsion at the focus, which so loaded down and vitiated his hypothesis as to cause its rejection. This result was similar to that of Le Sage's speculations and those of the Ptolemaic astronomers, each thus working out the falsity of his respective scheme by superadded complications to readjust the theory to the progress of criticism or of observed fact.

By attributing finite magnitude to the atomic mass, however, Boscovich's difficulty disappears, as I had the honor of pointing out before this Society some ten years ago. This may be deemed a violent hypothesis in regard to a positive discrete simple absolute, as the atom is presumed to be, but parallel difficulties inhere in any other finite supposition, as, *e. g.*, a sphere of repulsion. Under my provisional assumption, the way out follows from an elementary proposition of Newton's, and it does not demand the gratuitous change of law or of continuity involved in the resort of Boscovich. The movement of a gravitating particle under stress of a center of gravitative force would be in all respects as the great 18th century mathematicians have demonstrated, until the margin of the particle reached the attracting center, where, if we suppose the attractive virtue to prevade the particle equally throughout a certain finite volume of mass, however minute, as gravity does the mass of a sphere, the maximum of attractive force would be attained; for, as Newton has shown, homogeneous spheres are controlled under gravity by a law of force varying directly as the mass and inversely as the squares of the distance between their center of mass and the attracting center, at all points *beyond* the surface, and directly as the distance between the said centers *within* the surface; so that, after passing the surface, the attractive center must proceed onwards to the gravitating center of mass (relatively), not by a force increasing to infinity, but by a force decreasing to zero, after passing the maximum, since it is balanced at the center by opposing stresses.*

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* Let M be an exaggerated particle of mass and C a fixed center of gravitation external thereto. Newton proved that for all positions outside of a

A similar law of attraction prevails between two gravitative particles when both are similarly endowed with finite spherical volume and mass, excluding the idea of impenetrability (which is not a necessary attribute of mass), the Newtonian law being the product of the masses divided by the product of the distances $\left(\frac{Mm}{dd}\right)$* for outside positions.

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gravitating homogeneous spherical mass the stress is precisely as though the whole mass thereof were concentrated at the center of said sphere, and varies directly as the mass and inversely as the square of the distance between the said center and the fixed center of gravitation; *i. e.*, $G \propto \frac{M}{d^2}$. The maximum of gravitating force will here be at the surface, where *d* is minimum. He also proved that at all points within a homogeneous gravitating spherical concentric shell a gravitating particle is uniformly affected by balanced attractions. Hence, the stress for any smaller concentric sphere is $g \propto \frac{m}{r^2}$, *m* being the smaller spherical mass and *r* the reduced radius. But since homogeneous and similar masses are as the volumes, and similar volumes are as the cubes of the homologous dimensions,

$$m \propto r^3. \therefore g \propto \frac{r^3}{r^2} \propto r.$$

The maximum of gravitating force is here also at the surface, where *r* is maximum.

* I write the formula this way because it is possible that we have been in error all along in regarding the denominator as a radial space relation, as implied when we write it $\frac{Mm}{d^2}$. In discussing the deflection of the particle under gravity, Newton, for mathematical simplicity, treated it as governed by a fixed attracting central force, and in testing various relations found that the radial space relation gave the true path of the planetary bodies under the immense preponderating influence of the sun's mass. The fixed center of attraction is, however, a mathematical, not a physical, condition, and can only be realized by making $M = \infty$, when we get a form of expression which does not give a law of force. I think it possible that the relation is a mere reciprocal distance relation, since the stress is mutual for the masses and each is equally distant from the other. The inverse form of the relation, moreover, may arise from our subjective way of viewing distance, as measured outwardly from ourselves, since we have to go from here to yonder. It is possible to look upon the relation as really one of contiguity or nearness, and by placing $\frac{1}{d} = c$ we get the cosmical law of gravitation as *Mcmc*. This, however, would not be a useful formula, since we are not accustomed to expressions which attain maximum value with minimum magnitude.

For positions of encroachment the law is more complicated, and forms an interesting field for mathematical discussion. Where three or more atoms are superimposed the problem becomes too complex for discussion. It is noted, however, that such compound atom, if quiescent from extreme abstraction of heat, would be in a condition of elastic equilibrium, ready to respond like a bell to the slightest disturbances. In all these cases of interpenetration the law of stress would be finite and diminishing, and if the line of encounter should chance to be a right line through their centers (a condition infinitely rare in actual occurrence), they would continue on or repeat according to energy of approach; while upon any other lines of approach orbital relations would supervene, in modified curves of the second order, either hyperbolic, parabolic, or elliptic, according to velocity, and with or without partial penetration, according to nearness of approach.

Boscovich, however, did not adopt this solution, although within his reach. The problem of the action of a gravitative particle as controlled by an attractive center has several aspects of statement, which may be confined to four, for practical investigation. In the first, where the particle is assumed to be without mass, no discussion is possible, for the two suppositious points instantly assume the same locality, and end the relation. In the second, where the particle is endowed with inertia but not magnitude (and the attractive *locus* fixed by postulate), the element of motion enters, but infinite terms appear in the equations in the limit, forbidding interpretation. Thirdly, when we attribute finite magnitude to the gravitative particle for gravitative pervasion, as in actual spherical masses, no infinite terms appear, and we get an intelligible mathematical discussion, with planetary results for exterior positions, and pendulum results for interior positions, as I have heretofore demonstrated; and lastly, when both the gravitating *loci* are invested with similar attributes of volume and of mass (excluding extraneous notions of ordinary collision and repulsion from the problem), the results are similar to those of the third hypothesis. I do not introduce any of the mathematical discussions here, as the dynamics of the particle have been fully treated by mathematicians, though I am not aware that any of them have pursued it to physical conclusions.

It is not likely, however, that there is any matter so simple as this modified Boscovichian atom; that is, which can be identified.

All the matter we know of is already compounded and highly organized. The ideal simple molecule would consist of a single pair of such atoms, bound to each other in orbital relations of more or less eccentricity, including the extreme rectilineal form of simple pendulum-like oscillation through one another's centers; and it is a most significant fact that spectroscopic observation of all incandescent matter shows atomic matter to be in this state of transverse or orbital oscillation with inconceivable but synchronous rapidity without regard to range, according to the pendulum law of stress varying directly as the range of oscillation, discovered by Galileo. Any theory of the simple molecule must take cognizance of this observed fact. Another cognate fact is that the law of elastic cohesion manifest in all elastic tensile action—"*ut tensio sic vis*"—is a parallel law of stress, as illustrated in the spring balance weighing scale, the spring dynamometer, the isochronous spring governor, etc., and is a function of molecular and ultimately of atomic force and distance.

If the atom is really thus characterized, the repulsion or resistant property experienced in matter becomes worthy of investigation, since it drops out as the primitive affection or disaffection postulated by Boscovich. I have shown that it is not necessary to oscillatory motion. We must admit that the notion of rebound or recoil, in the ordinary sense, between simple atoms possesses difficulties. No less does the idea of plasticity or destruction of momenta. Consider what is involved in the hypothesis of two absolutely hard, rigid, unparticled, homogeneous spherical bodies of any magnitude at all, if possessed of mass, meeting on a rectilineal central line of motion. We know what would happen in case of ordinary spherical elastic masses or aggregations of molecules. Such merely undergo, first, apparent contact, then compression, deformation, strain, accumulation of stress, retardation of velocity, momentary arrest, acceleration on new lines of departure, relief of strain, recovery of form, redistribution of momenta, and final resumption of uniform velocities, with relative motion inverted and aggregate energy of motion unimpaired, unless permanent distortion and heat have absorbed a portion. All this complex action is involved in the term elasticity. None of this could take place with simple undifferentiated particles, unless we invent for them a mystic atmosphere or cushion of repulsive capacity surrounding the *locus*, as Boscovich was forced to do by logical conclusions. Without this, contact would be absolute and instantaneous at first impact. As hardness involves impenetrability, absolute destruction of motion on the instant must ensue; that is, motion and no motion at consecutive instants of time; a discontinuity unknown to experience, and known to be inconsistent with the nature of motion and of time. This argument from breach of continuity is due to Leibnitz. Conversion into heat motion is excluded, heat being a mode of motion of the entire atom. Moreover, the destroyed motion has to be recreated instantaneously in new directions, for destruction of energy cannot be postulated. This geometrically angular motion is also unknown to experience, for all deflected bodies pass by continuity from motion in one direction into a new direction, and, so far as we can see, must do so. These discontinuities in translatory relations are therefore put aside, not because they are inconceivable, but as illogical and non-experiential. Simple repulsion by contact without occult intervention is a false suggestion, and we find that we get the pseudo-conception from our false observation of what occurs in the collision of sensible masses, somewhat as we make a false observation and generalization about material continuity, or about tension, from a superficial perception of matter; thus creating concepts from supposed experience which can have no true objective counterparts. I shall recur later to a possible derivative basis for repulsion.

It is remarkable that to Newton we owe the final establishment of the majority of those fundamental and universal truths which by simplicity and generality seem to touch the absolute; that is, more than to any and all other philosophers combined. Thus, of the six ultimate generalizations, four were formulated and placed on an impregnable basis by Newton: the three laws of motion and the law of gravitation. All of these were inconceivable when first promulgated, were hotly controverted on the metaphysical plan, were finally established experientially, and are now generally accepted as axiomatic by the modern mind, except for sporadic reversions which appear now and then to deny their actuality and reassert their inconceivability. The remaining two universal inductions are the collective group of axioms formulating the relations of extension—the only enduring remnant of the Greek philosophy—and the law of the conservation and unity of energy, unperceived in Newton's time in its generality, though taught as a dogma by the

Cartesians. These also are still held to be inconceivable by certain disciples of metaphysical methods and axiomatic by others. Such mental attitudes should lead us to believe that simplicity has been arrived at in all these cases and the boundaries of explainable knowledge reached, where inconceivability necessarily begins.

It has been said that paradox is born either of confusion of thought, or of knowledge, or confusion of statement arising out of the imperfection or subtlety of the verbal vehicle of thought. Thus, as Clerk-Maxwell points out, the celebrated arguments of Zeno of Elea, establishing the inconceivability of motion, represented in the paradox of Achilles and the tortoise, were unanswerable and unanswered until Aristotle showed, some half century later, that duration is continuous and incommensurable by numerical methods in the same sense that extension is. The old logical dilemma of the irresistible force encountering the immovable body was insoluble to the Greek mind, both from lack of physical knowledge and lack of verbal clearness of statement. The acute sophist knew not the nature of force, the constitution of bodies, the conservation, transformation, and dissipation of energy, and consequently knew not the refuge and escape from the dilemma contained in the perception of the conversion of molar energy into heat energy, expansion, and dissipation. The resources of verbal subtlety and of inner consciousness failed, as they always do. Something of the same difficulty remains in modern problems, where observation and strict verification are, from the nature of the problem, inapplicable, or where the confusion arises from the still-existing imperfection of language, or, again, where generalizations, both clearly made out and clearly formulated, have not passed into the instinctive popular apprehension. The modern dilemma of the inconceivability of infinite or finite space is, I take it, due to the metaphysical form of the statement. For when we reflect that the ideas of immensity and of infinitesimal resolvability are but abstract generalizations of the merely relative continuities, extension, distance, and dimension, which are in their turn but abstractions of the sense-perceptions, form, translation, and volume, the statement becomes intelligible and entirely conceivable, and I think, though with deference, saves geometry; that is, the universality of that system of inductive postulates regarding the relations of extension and inferences therefrom, known as geometry to the Greek philosophy, but now named Euclidean by certain analysts whose so-called geometry is symbolic. Geometry is therefore able to deal with all aspects of extension, without regard to limit, in spite of some infirmity in the Greek method, for scale cannot affect the generality of extension relations, and abstract unconditioned space is not an entity but a mere negation, concerning which relative propositions are unintelligible. A false philosophy regarding space is at the root of all modern heresies concerning geometry and mensuration, founded in misapprehension of the Euclidean inductions or generalizations.*

The first law of motion is but the formulated recognition of inertia, which is only manifest in conjunction with motion, actively or passively. It was known to Galileo, and laid down by Descartes as a law in his Principia. It is a cosmical truth, bound up with the absolute nature of mass and the true relations of extension, which correlates the whole fabric of dynamical knowledge with rectilinear geometry, curvilinear motion being demonstrably not a simple state of conservation under inertia, but a resultant of multiple forces. The simple action of mass under the first law of motion, if undisturbed, furnishes the absolute unreturning rectilineal path which overthrows all speculation about possible ideal spaces. I here recall a book written by a learned American of Philadelphia—learned, that is, according to the mediaeval standard of the colleges—and published only during the past year, en-

* There are two opposite though similar forms of error in the assumptions regarding space. The first is that space is a specific or perhaps generic entity or objectivity *per se*, possessed of conditions and attributes, like substance, such as dimension (in several), differentia in locality, figure, as curvature, etc. (hence necessarily finite), and only uncognizable by us simply for lack of perceptive faculties to correspond. This is the fundamental error, as it seems to me, of Riemann and Lobatschewsky. The second is that of the older Cartesians, who viewed space as but the mere attribute or synonym of substance, and inconceivable apart from it, so that bodies separated by void space would be absolutely in contact without regard to distance. Both of these speculations are purely metaphysical, and non-experiential, the latter resulting from the old scholastic method of syllogistic deduction from primary postulates of verbal definition, and the former from similar inferences from the forms of the analytical logic of symbols, the use of which is still in the scholastic stage. Like Zeno's paradox, these merely intellectual difficulties should be removable by intellectual processes.

titled "An Examination of the Philosophy of the Unknowable, as expounded by Herbert Spencer," wherein he naively lays down the first law of motion as unintelligible except by appulsion. Motion, he says, in the absence of propulsion is inconceivable. I have no space here to reproduce the explanation evolved out of consciousness by this reasoner to account for the action of a ball struck by a bat after leaving the bat. It resembles in ingenuity and gratuity some of the inventions devised to explain gravity. The notable thing about it is that here, at this date, is a mind of good caliber, informed in the higher schools of learning, which is still of the mental period of Aristotle; a mind which has evidently never apprehended inertia, nor heard of the great contributions to knowledge made by Galileo and Newton, by which philosophy was entirely revolutionized.

The second law of motion, regarding the independence and coexistence of motions, on which we occasionally see comments in the metaphysical vein controverting its possibility, has long been established experientially. Its early experimental proof is attributed to Galileo. Yet I recall a pamphlet written and published only during the last year by a learned German at Leipzig, the theme of which was that "the sun changes its position in space, therefore it cannot be regarded as being in a condition of rest." This, he concludes, overthrows the entire fabric of Copernicus, because the planetary orbits in such case cannot be closed.

The third law of motion is but formulated reciprocal stress, in its modes of compulsion and repulsion, through which mass acts on mass to redistribute motion by what appears to be necessary law. The stress is necessarily reciprocal, since there is no *point d'appui*, or fixed fulcrum, in the universe.

We have thus been brought to the boundary of the absolute, where all is inconceivable until found out, and where the simple data are unexplainable. All examination seems to continue to point to mass and weight as the ineffable simple insignia of substance standing on this limit. We must accept something as elementary fact; what shall we find more elementary? Repulsion is still debatable; for, if we make an issue between repulsion and compulsion as contradictory primary attributes of the same essence, or untenable in conjunction for artificiality, by far the greater difficulties attach to the former, some of which I have already alluded to. The profound mind of Boscovich was forced to accept repulsion as a primal quality, but in deference to the physical hypotheses of his time, he overloaded it with complication. This has been weighed in the balance of philosophical judgment and found wanting. I have intimated that there are possible grounds for surmising that it may not be a simple property of the atom, but a mere mode of distribution of energy dependent on composition of motion of atomic mass after change of sign, *i. e.*, a mode of *vis impressa* after exhaustion of the space relation; for, mathematically, the hyperbolic lines of approach and recession of two atoms under the high proper motion characteristic of the atom, and on lines not directly central, would be similar, at sensible distances, in their asymptotes (which would be the practical paths), whether the deflection were due to attractive or repulsive stress, though acceleration and retardation at the passage of the infinitesimal focus would be inverted.*

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* It is well known that for any finite system of two particles controlled by gravity the lines of movement are closed curves of the second order, of more or less eccentricity, about the common center of gravity, which, for equal masses, would be midway. For an infinite system under the same conditions the orbits are parabolic, but for a system to which the particles enter by extraneous motion the lines of movement are hyperbolic, thus:

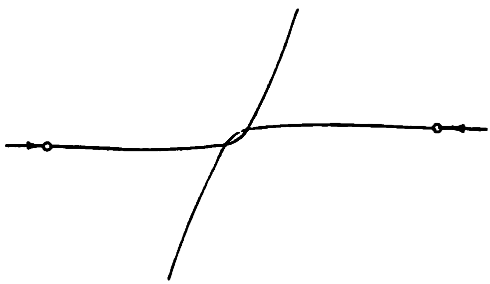

FIG. 1.

Now, under repulsion, the lines of motion are seen to be similar, A B, D E, Fig. 2, being asymptotes of the hyperbolas representing the two paths at sensible distances:

It therefore seems to me immaterial to result which of the two modes of passing the infinitesimal focus is the true one. In either case the distance at passage is infinitesimal, and the force may be as near infinity as the facts require it to be assigned. The normal or rectilineal encounter is here excluded from supposition. In that case, under repulsive stress, as postulated by Boscovich, the recoil would be rectilineal and opposite, without breach of continuity. Under attractive stress, with finite volume of the atomic mass, penetration would ensue as before shown; but without dimension or repulsion we have an insoluble condition, although the occurrence would be infinitely rare. Only one pair of elements is here considered. In all real encounters, whether of masses or molecules, the effect is a vast resultant, but should not be different in kind from that of the elements; that is, hyperbolic or expansive between alien systems under motion. As the number of elements ordinarily engaged could not be represented by any numerical places of arabic notation for which we have names, we see the hopelessness of stating the problem mathematically. I therefore do not presume to

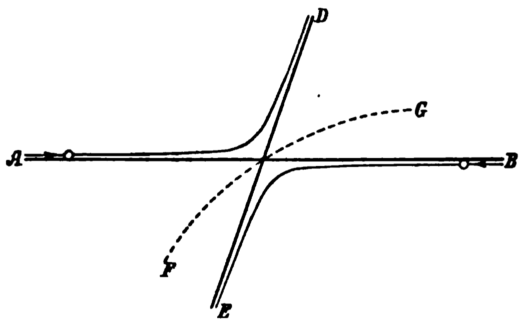

FIG. 2.

This encounter represents only one element of the molecule, of which myriads are engaged at every recoil of molecules, not to speak of solids. It is thus seen that the mesh constituting the molecule is ordinarily impenetrable to other meshes. If the curve F G be allowed to represent the outline of the molecule, the limb of the solid to which it belongs, say a buckshot, will be represented by the Sierra Nevada, or the Andes, and its diameter would be measurably represented by that of the earth, as approximately shown by Sir Wm. Thompson in the case of a drop of water.

offer this as an explanation of repulsion, and I confess that to me repulsion is in its mechanism incomprehensible. We know the result experimentally, and that is resistance to penetration, and reaction at insensible distances on an undefined boundary which begins prior to contact and increases in a high exponential ratio as approximation progresses. The contact boundary of any solid—even the smoothest and hardest—resembles the astronomical limb of Jupiter in geometrical indefiniteness. The contact transmitter in the telephone, the whole range of whose phenomena occurs under pressure and so-called contact of varying degrees, illustrates how relative a thing is contact. Under high velocities the distinction between solids, liquids, and even æriform bodies entirely disappears in respect to repulsive reaction, though this is the most sensible distinction between them under low velocities.

We may, therefore, adopt the conclusion that if any of the apparently simple properties of the atom are to be thrown out as derivative and secondary, presumption points to repulsion as the complex one. We could possibly account for phenomena in a universe bound together by purely tensile stress, but most of the sensible phenomena of solids—cohesion, affinity, tenacity, etc., including nearly all of statics—remain hopelessly unattackable problems under a hypothesis of pure repulsion, like that of Le Sage, or Preston. It is to be noted that the kinetists who freely postulate repulsion and appulsion, without analysis, as a primordial fact, but reluct against compulsion or tension, are forced to the invention of the most complicated and gratuitous mechanism and media to explain the phenomenon of gravity, and then without attainment of result. Le Sage's atom is too complicated, even without his suppositious or extra-mundane operative machinery; and the vortex atom is but a mere analytical expression for an unproducible condition in a figmentary mathematical plenum.

The thesis that conservation is the characteristic by which we identify objective existence will not bear the test of examination It is only in the most recent times that such a quality has been known or imagined, and its establishment, both as to matter and energy, is justly viewed as the triumph of modern philosophy. The evocation of matter from nothing and its relegation to nothing, even by the finite will of a wizard, was ever a common and universal notion, which did not at all impair the belief in its present reality and substantiality. We have only to go to Apuleius for this, and it is doubtful if even now the notion of the indestructibility of matter is anything but a scientific conviction, for do we not see numbers of our contemporary fellow-citizens meeting together frequently in our midst to witness feats of materialization out of nonentity by powers akin to those of the sorcerer, without an idea of incongruity? Nor has the essentially modern doctrine of the conservation of energy anything to do with the belief in its reality. Few people apprehend it even now. No philosopher understood it a hundred years ago. Its verity rests on a sufficiently general inductive basis, from the refined and exhaustive experiments of Joule, and the theoretical conclusions of Mayer and Clausius, and it is accepted in the same sense that the law of gravitation is accepted. But the duality of matter and energy to the exclusion of force is a verbal shift, the assumption of which removes no difficulty. Matter, the object, remains unexplained; and energy, the phenomenon, becomes segregated and unintelligible. Energy, in fact, is but mass in phenomenal manifestation, being a product of triple factors, two of which—translation and speed—are not things, but variable and evanescent conditions, and, taken together, constitute motion. Mass is the absolute or persistent factor, but the evanescent character of the variable component—motion—would render the entire phenomenon—energy—apparitional, were it not for the distance relation involved in motion, which, under the same inscrutable agency which modifies and saps the motion renders it potential upon change of sign. This agency, the dynamical source of the manifestation, being central to mass and likewise persistent and constant, renders the positive and negative potentialities of movement constantly equal, and the actual and potential energies consequently complementary, from which energy gets its character of conservation.

Energy cannot therefore be that other reality of existence (besides matter), since force is clearly the one reality at the bottom of the manifestation of both, to whose persistence and resistance to change, except through transformation, the conservation of both is due. This one reality is, in its triple aspect of causation, (1) attraction—the source and modifier of motion; (2) inertia—the conserver of motion; and (3) repulsion—the distributer of motion; or, more correctly, in its aspect of quality: (1) *vis centripeta*—the power of mutual control across distance; (2) *vis insita*—the power of persistence in state of motion impressed; and (3) the distributive power of imparting and acquiring motion by transfer, at minimum distance, which may be called *vis partitiva*, the result of which is Newton's *vis impressa*. Matter thus comes into the world of phenomena by the simple presence of other matter, permitting the exhibition of these comparisons and interactions, involving the conditions of contiguity, distance, position, translation, direction, succession or sequence, and time-rate for the continuous increments, decrements, successions, and uniformities, all bound up in the compound variable continuity—motion. With motion and distance comes the dependent phenomenon—energy—active and potential, which should be a constant, the numerical units of mass being constant throughout immensity, provided the sum of the motions, potential and actual, be constant. This the dynamical theory deduces from the fact of central force (for without force potential motion is ridiculous), and the thesis of the conservation of energy is a dynamical truth or nothing. It is therefore all the more extraordinary that certain kinetists, who reluct against central force, should have selected, out of all the manifestations of the universe, the variable and conditional product—energy—to be the one reality or objectivity, aside from the undefined hypostasis—matter—as a primordial simple fact at the basis of phenomena. It has been mathematically demonstrated by Mr. Walter R. Browne (London Edinburgh and Dublin Philosophical Magazine, January, 1883, p. 35) that the conservation of energy is true if the material system is a system of central forces, and is not true if the system is anything but a system of central forces. In fact, the ordinary theoretical proof of the principle of the conservation of energy assumes the forces acting to be central forces, *i. e.*, reciprocal stresses between units of mass, as recognized by Clausius in his Mechanical Theory of Heat. Moreover, the entire body of kinetists, who have aimed to supersede gravity or central force, have freely assumed an extramundane supply of motion and energy without regard to conservation, and it is notable that every hypothesis for this purpose yet broached involves the constant expenditure of work without recovery, and postulates the accession of energy in infinite influx from some occult source, of which only a small portion relatively is available or manifest in observable phenomena, thus violating all three of the canons of philosophical ascription—true cause, sufficient cause, and least cause. Such is the power of conception of the unknown in endeavor to explain the inconceivable known.

If the dynamic hypothesis of perpetual transformation of energy could be established as a universal induction, with as much generality, *e. g.*, as the statement of the law of gravitation, it would establish and confirm that law, by Mr. Browne's demonstration, as something more than a law, to wit, the necessary constitution of matter as a system of central forces and nothing more, substantially as conceived by Newton and elaborated by Boscovich. At present it is but a dynamic induction, but the theory of gravity is no more. Our appliances are material, and we can deal with molar forces, but only indirectly and inferentially with those which are atomic. Conservation is indubitably true of energy in the mechanical and molar sense, under the laws of dynamics and the persistence of force. It is, also, experimentally true, so far as we can trace it, of those less understood forms of energy which are molecular or atomic, the establishment of which was the great glory of Benjamin Thompson, Clausius, and Joule as to heat, and of a multitude of observers as to electrical energy. We infer it as a general truth of these energies (formerly known as imponderables, since they are not manifestations of matter in the concrete), from the fact of their convertibility with other modes of energy which are undoubtedly dynamical, and also from the intimate connection of electrical energy with one of the specific exhibitions of central atomic force—magnetism. Such clews create a warrantable presumption that the phenomena in question will all ultimately be classified among the modes of atomic mass and motion, inductively as well as hypothetically. Possibly in the investigation of these evanescent modes of energy the missing simple particle may come to light. Provisionally, we are entitled to rank them among the mechanical modes of energy, as products of the same material forces, assuming, until the contrary is proved, that some form of matter is concerned in manifestations so correlated by conservation with undoubted material activities.

In including the imponderables within the general dynamical law of conservation, we have to take account of the phenomenon of dissipation, first pointed out by Sir William Thompson. It is true that heat (as well as electrical energy) is strictly correlated with and interconvertible with energy of mass motion, as before stated, but in its final form energy seems to take leave of matter altogether, so far as our perceptions can follow it, and disappear as a material phenomenon (though liable to reappear wherever matter is encountered whose particles are deficient in a like species of atomic motion with that which disappeared; which fact indicates that atomic mass is still a factor, with its inherent property of persistence and transference). The earth and all upon it is radiating heat energy away into space at the constant rate of 500° F. of absolute temperature, more or less; the sun and the visible stars at the rate of many millions of degrees. Much energy also passes off in the luminous form. Of electrical and actinic energies we know less, and of some we doubtless know nothing. This amounts to a constant drain of the dynamical supply of energy. These final forms, the radiant energies, have a remarkable specific high cosmical velocity of their own, which is a function of something not material, or at least not molar. It is supposable that, in addition to the dynamical source of motion from central forces, and the contraction of systems in dimension which supplies dissipation, there may be an inherent and primordial store of atomic motion. The high proper motion of some of the stars, beyond what can be accounted for on dynamical principles, and the inexhaustible and enormous supply of radiant energy from the visible stars, have afforded grounds for such a surmise, but these speculations do not belong to the domain of mechanics.

And here we must bear in mind that the dynamical theory, in placing these assumed agencies and modes of interaction in causal relation to phenomenal motion, by no means predicates or can predicate anything concerning absolute motion or its cause. The lack of this distinction may have proved a stumbling block to some in comprehending the idea of force. Were it not for the observed dissipation of energy no system could become contracted in dimensions a particle by the interactions of material forces, nor is there now any known way by which the material system can be expanded in dimensions except by the accession of motion from extra-mündane sources, which there is no scientific mode of ascertaining. The sum of motions under the action of forces remains the same, and any change would imply creation or annihilation, which is not ascribable to a material agency. Primordial dimension remains as inscrutable a fact as ever, and primordial motion an unsolved problem.

In conclusion, I know nothing of force except as a manifestation of matter, and nothing of matter except through its manifestations. It is substance that interacts with substance, so far as we know, always reciprocally, and force is but the convenient translation of the terminology invented by Newton to designate these several species or modes of action, in the word *vis*, with its appropriate adjective. He was arraigned by the Cartesians (and virtually is by their modern representatives) as the reintroducer of occult qualities into philosophy, but his statement was "*hypotheses non fingo*," and to a similar charge brought against him by Leibnitz he pertinently replied that it was a misuse of words to call those things occult qualities whose *causes* are occult though the qualities themselves be manifest.

I have adopted gravity as the type of central inherent force—*vis centripeta*—but I would not thereby be understood as excluding from the category of material forces any and all other modes of tensile or constraining force which may be hereafter made out as specific, by the elucidation of such phenomena as affinity, cohesion, tenacity, elasticity, ductility, viscosity, capillarity, polarity, magnetism, etc., now so little understood, any more than I would exclude any form or mode of energy which may be observed, from the category of material phenomena. The Newtonian doctrine of force would not be impaired by such discovery, and its strength lies in the fact that it as readily includes static phenomena—that despair of the kinetist, who has no imaginable hypothesis by which to range them under a form of motion—as it does kinematical phenomena. Statical force (Newton's *vis mortua*) cannot be ignored in a theory of force. The straw that breaks the camel's back—the very lightning that crashes through the sky—are familiar examples of its power made manifest. Its reality may be exemplified by suspending two heavy balls of equal weight at equal heights—one by an elastic cord, and the other by a tense string. The difference of effort required to displace the two vertically upwards, which can be measured, makes sensible the difference between the two forms of balanced statical forces. In the one case the antagonizing force is suddenly withdrawn, and in the other gradually. Wherever strain exists—and it is everywhere—there force is as certainly present as when it becomes manifested in a stress relieved by motion and measurable in terms of energy.

Let us, then, give up the standard of *a priori* conceivability, in view of its many historical failures, and adopt as possible that which is provisionally ascertained. The "ego" and the "cogito"—Cartesian starting points—have proved barren and irrelevant in Philosophy. True Philosophy is concerned with objectivity. The data of consciousness, mainly acquired in infancy or in the womb, are blind guides. Many an ego, whose brain was his cosmos, has run through his brief subjectivity, but the order of nature endures. The same facts are continually observed, verified, recorded, and rectified, but the observers change. Their intelligent observations add to the sum of knowledge. This is all the proof we need of objectivity, and all we will get. The insoluble difficulties of Philosophy have disappeared one by one since the happy thought of eliminating them by observation entered. The immortals are those who have successfully applied this method. It is only where observation fails that the insolubility lingers. Beyond the sphere of the knowable it will continue, in spite of introspection. How masterful is fact in the presence of the most intricate mental subtleties. The ball leaves the bat, in spite of the inconceivability. Galileo's plummet dropped from the moving mast strikes the deck and not the water, in spite of the inconceivability. The Earth returns in its orbit, to the second, in spite of the sun's rapid fall through space, and of the inconceivability. Two opposed horses can pull no more than one, in spite of the inconceivability. The guinea and the feather dropped in the exhausted receiver strike the plate together, in spite of the inconceivability. The isochronous pendulum swings through the widest arc in the same time as through the smallest, in spite of the inconceivability. The minute hand overtakes the hour hand, in spite of the inconceivability. The magnet draws the iron with undiminished force through all possible interpositions, in spite of the inconceivability. Could an exception be found, the perpetual-motion "crank" would work a greater inconceivability, by the instant contrivance of a power-generating machine.

We need not aspire, therefore, to remove any of the inconceivabilities of the external world. We must accept them as natural to the finite comprehension, as necessary to faculties which act by comparison, and above all as evidences of objectivity. On the other hand we should avoid that opposite error of the introspective school, of deeming that probable, or in any way connected with fact, which merely seems conceivable. I have shown that while the simplest truths have generally proved inconceivable until found out and established by genius, the greatest absurdities have had ready currency without a doubt of their conceivability. This all mythology shows. Such rubbish as "a thing cannot act where it is not," and "a body cannot move where it is not," or "a cause cannot precede its effect"—mere metaphysical assertions or subtleties in face of everyday fact—were stumbling blocks for ages. Such assumptions formed the basis of deduction in lieu of observation, and blocked the possibility of advance. And even yet, rigid deduction from the most hare-brained premiss, if the chain of deduction is sufficiently intricate, seems to possess fascinations over a verifiable induction, with many minds.

And now, if any ask, "*cui bono*" to the scientist, these philosophical inquiries and intricacies when he has the vast field of unexplored data still before him to occupy him, I answer, the queries of Philosophy are not only the main-spring and final cause of science (her first fruitful daughter and handmaid), but they, consciously or unconsciously, dominate the methods and results of science herself. Each investigator, even though in the domain of the most abstract of the sciences, postulates more philosophy than he is aware of; and with so much the more danger to final accomplishment if he assumes his philosophical basis without examination. It is the errors of giant minds that are dangerous, by their ponderosity. The infallibility of the master, Aristotle, seemed to make investigation useless, until the rise of parallel giants, like Galileo and Copernicus, stimulated a new conflict of opinion. And Descartes, though harmless from all his productions within the metaphysical domain, is dangerous by his very eminence and originality in science, which gives vogue and currency to his monumental errors. Although acquainted with the true law of motion, his scheme of matter evolved from consciousness would forbid all exhibition thereof. A grand geometer, he erected a scaffold for scaling immensity, and with unparalleled penetration perceived how a purely ideal logic, if general, would represent truth in a wholly dissimilar realm of deduction, if equally general. Strange to say, this grand and useful discovery has become the engine, in nihilistic hands, for overthrowing all the positive knowledge we possess—the achievements of two thousand years of human effort. Not only geometry—all that has survived to us of philosophical value from the antique world—but the basis of positive dynamics, as handed down from Galileo and Newton and Boscovich and Dalton, are apparently undermined, for all that gives them intellectual value—their certainty—unless an effort be made in the neglected field of philosophy. With strange inconsistency these advocates *par excellence* of the experiential origin of knowledge are found in the same breath promulgating as possible truth matters not only non-experiential, but not representable in ideas derived from or verifiable by experience, and avowedly originating not from inductive generalizations—the only source of knowledge—but in purely deductive processes in the old scholastic way, from logical premises of bald assumption. In a similar way, in the hands of the Greek sophist, language, a good servant, became a vicious master, and made a chaos of all ethical achievement. A remnant of knowledge, fortunately expressed, not in verbal, but diagrammatic logic—geometry—was left, but only to fall now by the hands of similar iconoclasts, armed with more potent destructiveness, in its full flower and fruit of twenty centuries of unmolested growth.

It is time, therefore, to get back to Baconian ground, and while using for its legitimate purposes the magnificent modern machinery of analytical investigation in the field of abstract continuity—extension, motion, duration—not attempt to conjure with it as a source of objective revelation, which no mere machinery can be. A scaffold of n dimensions is as useless to the geometer as to the architect. To assume matter as continuous, simply because of the possession of a potent engine for the investigation of continuities, is to repeat the practice of certain quack specialists, who are prone to diagnose nearly every form of disease as a variety of their own peculiar specialty. And to interview the symbols of a mathematical logic for the prime definition of a fundamental objectivity, like force, is to revert to a barren source of knowledge, by an obsolete process in philosophy, and bar all progress in anything but abstract technique.

The paper was discussed by Mr. W. B. Taylor and Mr. Kummell.

Mr. T. Robinson made a communication on

THE STRATA EXPOSED IN THE EAST SHAFT OF THE WATER-WORKS EXTENSION.

[Abstract.]

The shaft (23′ square in the clear) was begun in the bottom of an old sand-pit at a level of 131.5′ above tide. This sand-pit was excavated in the side of a hill; and recent cuttings have exposed the strata from the hill-top to the level of the top of the shaft. Thus we have a vertical section of 188.5′, extending from 171.5′ above tide (or 40′ above the top of the shaft) to 17′ below tide.

1. About 6″ of surface soil.

2. A layer of gravel in red clay, about 4′ thick, containing isolated bowlders from a foot to two feet in their longest diameters.

3. About 24′ of a mixed material, consisting mainly of sand and kaolin. The two are sometimes uniformly mixed; at other times they lie in separate masses of two or three feet in thickness at one point, and run down to as many inches at another. In short, the whole bed is a sort of "pell-mell" of sand and clay.

4. A bed of sand, about 10′ thick, generally sharp and clean, but varying from coarse to fine grains, and streaked with iron oxides, with pebbles near bottom of stratum.

5. A thin stratum of clay, about 2′ thick, varying in color from blue to red, and containing in spots fragments of lignite.

6. 2.5′ of sharp, coarse, clean sand.

7. 32.5′ of red clay, mottled with blue and gray, showing no lamination.

8. 5′ of sandy clay, mottled as above. Between this stratum and the clay above, there was no dividing line; the two beds blended gradually along their line of union.

9. A bed of gray, clayey sand, 6′ thick. In this bed occurred, on one side of the shaft, some masses of sandstone, somewhat more ferruginous than the surrounding sand, and on the other side a *tongue* of clean, red clay.

10. A bed of sand with its upper surface horizontal, having a thickness of about 1′ at one side of the shaft and 4′ on the other.

11. A stratum, about 2′ thick, of sandy mud, containing lignite. The laminæ of this bed were horizontal, while its upper surface fell from north to south at the rate of about one in eight.

12. 6′ of sand containing nodules of iron pyrites, isolated masses of lignite, and pockets of red clay.

13. A bed of fine, clean sand, containing here and there a little clay. This bed was 9′ thick, and gradually gave way to the succeeding bed.

14. A bed of sandy kaolin, 6′ thick, very wet and difficult to work. It was a regular mortar-bed in consistence.

15. A layer, 2″ to 4″ thick, of hard, ferruginous conglomerate.

16. 9′ of blue-grey clay, hard, compact, and possessing a very unctuous feel. This bed contained a bunch of rootlets, the first trace of organic remains below the lignite of No. 11.

17. A bed of clayey sand, streaked with red, blue, and grey, 7′ thick, and gradually running into the subjacent stratum.

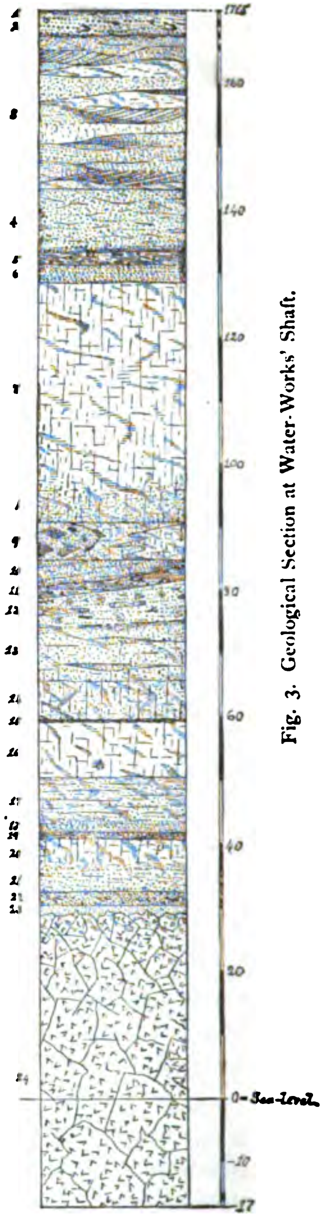

Fig. 3. Geological Section at Water-Works' Shaft.

18. A bed of clean, white, sharp sand, about 2′ thick. (These last nine feet were difficult to work. The material could not be shovelled, and was too sandy to pump.)

19. A layer of red sand about 1′ thick, containing on one side of the shaft a clayey sediment with lignite, and on the other a ferruginous conglomerate.

20. 5′ of blue-black, hard clay, running into a sandy sediment, and this, in turn, into the next stratum.

21. 3.5′ of clean, white sand.

22. 2′ of dark green, compacted sand, containing pebbles and lignite.

23. 1.5′ of fine, sharp sand, almost apple-green in color. Beneath this lay the irregular surface of No. 24.

24. Dark, coarse-grained, soft, chloritic rock. This rock could be easily removed by the pick to a depth of three feet, where blasting was begun at about twenty-six feet above mean tide. The rock grew harder as the depth increased for about ten feet, when it became a chloritic gneiss, and in general remained of that nature through about thirty feet to the bottom of the tunnel grade, or seventeen feet below mean tide.

255TH MEETING. JUNE 7, 1884.

Vice-President BILLINGS in the Chair.

Thirty-five members and guests present.

Mr. G. K. GILBERT presented a

PLAN FOR THE SUBJECT BIBLIOGRAPHY OF NORTH AMERICAN GEOLOGIC LITERATURE.

Mr. J. W. POWELL presented a slightly different plan for the same purpose.

These plans proposed to establish at the outset a limited number of divisions of the subject-matter of the literature and to simultaneously prepare a bibliography of each.

Mr. J. S. BILLINGS criticised the plans at length and advocated that which has been adopted for the indexing of the Library of the Army Medical Museum.

Other remarks were made by Messrs. ANTISELL, NORRIS, GOODE, E. FARQUHAR, F. W. CLARKE, HARKNESS, TONER and WARD.

The meeting announced for October 11 was informally adjourned, to enable members to attend a meeting of the Anthropological Society, and listen to an address by Dr. E. B. Tylor, of Oxford, England.

256TH MEETING. OCTOBER 25, 1884.

The President in the Chair.

Forty members and guests present.

The Chair announced the death, since the last meeting, of Dr. JOSEPH JANVIER WOODWARD, a former President of the Society, Gen. ORVILLE ELIAS BABCOCK, and Gen. BENJAMIN ALVORD.

Announcement was also made of the election to membership of Messrs. WASHINGTON MATTHEWS, STIMSON JOSEPH BROWN, TARLETON HOFFMAN BEAN, and ROBERT EDWARD EARLL.

Mr. S. M. BURNETT read a paper entitled—

ARE THERE SEPARATE CENTRES FOR LIGHT-, FORM-, AND COLOR-PERCEPTION?

controverting the theory which gives an affirmative answer to the question, and maintaining, first, that there is no white-light sensation that cannot be resolved into its constituent elements of color sensation; and, second, that the sense of form is an expression of the idea of extension as represented by the dimensions of the area of the retina impressed. The idea of form is not a purely visual sensation, but is based also on information derived from other sources.

[The paper is published in the *Archives of Medicine*, Vol. XII, No. 2, October, 1884.]

Mr. T. ROBINSON read a paper entitled—

WAS THE EARTHQUAKE OF SEPTEMBER 19TH FELT IN THE DISTRICT OF COLUMBIA?

[Abstract.]

At 3.20 p. m. of September 19 I noticed a peculiar vibration of the floor, table, and chair. I saw my ink shaking and heard the door of the room rattling. The table and chair rocked in a north and south direction. The sounds made by the door were at regular intervals of something less than a second each. My room is on the second floor of the Howard University building.

Immediately after the occurrence I inquired if other persons had noticed anything unusual at that time. One had heard a rumbling, another had felt the shock, and a third had both felt and heard it. The miners in the water-works' tunnel also heard a rumbling noise at about the same hour.

From the motion of my table and chair and the continued thumpings of the door I judge that the shock passed in the direction of the meridian, and continued from ten to fifteen seconds.

There was no local cause for the phenomenon, and I concluded that it was in some way connected with the earthquake that occurred in the West at about the same time.

Mr. PAUL remarked that the direction of the motion communicated to buildings by a slight earthquake shock is not a reliable index of the direction of the earth tremor. The azimuth, amplitude, and period of vibration of the buildings are functions of their structure rather than of the azimuth, amplitude, and period of the earth vibration.

Other remarks were made by Mr. H. A. HAZEN and Mr. ELLIOTT.

Mr. J. S. BILLINGS exhibited a collection of microscopes illustrating the evolution of the mechanical stage. The collection will be sent by the Army Medical Museum to the New Orleans Exhibition.

Mr. BILLINGS read a paper by Mr. WASHINGTON MATTHEWS on

NATURAL NATURALISTS.

[Abstract.]

It is easy to understand that a savage may be well versed in the knowledge of animals and plants which contribute to his wants, but it is a matter of surprise that with equal care he acquires and disseminates information about creatures which he does not use. I have never yet failed to get from an Indian a good and satisfactory name for any species of mammal, bird or reptile inhabiting his country; and I have found their knowledge of plants equally comprehensive. It is true that not all Indians are equally well informed in this respect, but, as a class, they are incomparably superior to the average white man or to the white man who has not made zoology or botany a subject of study.

There is a prevalent impression that Indians are unable to generalize; and a paragraph goes the rounds of ethnological treatises to the effect that the Chatas have no general term for oak tree, but only specific names for the white oak, the black oak, the red oak, etc. This impression is entirely erroneous. The Indian is as good a generalizer and classifier as his Caucasian brother. His system of classification does not fully coincide with that of the white naturalist, because his system of philosophy leads him to base his groups upon a different series of resemblances, but his arrangement is nevertheless the result of a process of generalization.

Mr. Ward remarked that his own experience fully sustained the statements of the paper in regard to the botanical ignorance of white men, but less fully in regard to the accuracy of Indian observations. When collecting plants in Utah, he had found that Piute boys and girls gave names to nearly all his specimens, discriminating allied species; but in collating the Indian botanical names recorded by others, he had been led to suspect that certain discrepancies arose from failure to recognize the same species in different stages of development.

Mr. Mason said it is a canon of anthropology that things seem marvellous to us only when we do not understand them, every human phenomenon being governed by law. Our ignorance in regard to wild animals and plants is to be explained by the fact that our activities do not bring us into close relation with them, whereas the savage is dependent on them for sustenance. The market-women who bring herbs to Washington have names for them all, and ignorant mechanics handle technical terms of their craft with great familiarity.

Mr. Dutton said that his own acquaintance with the Navajos made him prone to believe that they diagnose species of plants, but he questioned their powers of generalization.

In illustration of Mr. Mason's remark that familiarity is conditioned by contact, he related that rural rambles had made him when a boy so familiar with the fauna and flora of his district that he knew a name for every prominent species. As a man, he had been occupied with other and different matters, and had lost this familiarity.

Mr. WELLING admitted that the Indian was an acute observer, but questioned the propriety of calling him a naturalist. As illustrated by the paper, his methods of interpretation are metaphysical, not scientific.

Other remarks were made by Mr. HILGARD.

257TH MEETING. NOVEMBER 8, 1884.

Vice-President BILLINGS in the Chair.

Forty-eight members and guests present.

Mr. BILLINGS, on behalf of the General Committee, reported the following resolutions:

Resolved, That this Society receives with deep regret the announcement of the the death, on the 17th of August last, of Dr. JOSEPH JANVIER WOODWARD, an ex-president of this Society and one of its original founders.

Resolved, That this untimely death has deprived science of one of its most energetic, patient, and skilful workers and this Society of one of its most efficient and distinguished members.

Resolved, That in our sorrow for this affliction we have some consolation in the knowledge that his long and great suffering is at last ended and that the fruits of his unceasing labors for the last twenty-five years remain for the benefit of the world and as an enduring monument to his memory.

Resolved, That a copy of these resolutions, duly authenticated, be forwarded to his bereaved family.

In presenting these resolutions, Mr. BILLINGS spoke briefly of Dr. Woodward's work and his characteristics as a scientific man, eulogizing his accuracy of observation, his delicacy of manipulation, his conservatism as a theorist and as a critic of new ideas, and alluding to his delight in teaching and his interest in, and affection for, the Philosophical Society.

Mr. POWELL spoke of his remarkable acumen and his conspicuous mental integrity. Mr. GIHON spoke of his boyhood; Mr. TONER of his ability as a practitioner; and Mr. E. FARQUHAR of the impression of great force conveyed by his presence and conversation.

The resolutions were unanimously adopted.

Mr. C. E. DUTTON made a communication on

THE VOLCANOES AND LAVA FIELDS OF NEW MEXICO,

his remarks being illustrated by photographic lantern views, and by a map exhibiting the boundaries of the region usually termed the Plateau country.

[Abstract.]

Beginning at the north, the boundary of the Plateau country runs along the southern base of the Uinta Range to the junction of the latter with the Wasatch; following the eastern base of the Wasatch southward it strikes off towards the southwestern corner of Utah; thence turning due south it crosses the Colorado river, and gradually shifts its course to the southeastward, preserving this direction for nearly 400 miles and far into New Mexico; here it rapidly turns north northeastward, reaching into the Valley of the Rio Grande, and follows the western bank of that river nearly or quite into Southern Colorado; here the course of the boundary is somewhat indeterminate, but is, in a general way, first northwestward, then northward to the place of beginning. The western and southern border of the Plateau province is usually sharply defined; the plateaus end generally in great cliffs suddenly terminating the horizontal strata, and the profiles drop down upon the rough, irregular topography of a type peculiar to the Great Basin. The eastern border of the Plateau province is by no means so definite; the features peculiar to it pass rather by gradual transition into those characterizing the Rocky Mountains of Colorado.

Among the many geological features of this wonderful region, the volcanic masses are not the least interesting. Volcanic action has prevailed there upon a grand scale, and it may be first noted that volcanic rocks predominate around the borders of the province. The interior spaces, while not wholly devoid of them, show but a very small amount. The region of the High Plateaus of Utah, which lies upon the western or northwestern border, discloses a very large mass of lavas, erupted chiefly during tertiary time, and representing almost continuous activity from the eocene to the quaternary. Proceeding southward, we are never out of sight of eruptive masses, and in the Unkarets, on the border of the Grand Cañon, we find many scores of old and young cinder-cones and some considerable lava-fields. In the San Francisco Mountains we also have a vast field of volcanic rocks, and thence southeastward they augment in volume and area until at the southernmost extension of the Plateau country they become indeed immense. Still following the boundary northward into the Valley of the Rio Grande they are found abundant, and a singularly interesting field is presented in the neighborhood of Mt. Taylor. The speaker was engaged during the past summer in the geological examination of the Mt. Taylor district, and it is of the striking features there presented that he designs especially to speak.

Mt. Taylor is an old volcano long since extinct. Its altitude is about 11,400 feet above the sea. It stands upon a high *mesa*, from the summit of which it rises as an ordinary volcanic cone of considerable magnitude—much larger than Vesuvius, much smaller than Ætna. Its lavas are rather monotonous in type, so far as external appearances are concerned, consisting probably of basalts and andesites. The *mesa* upon which it stands is of great extent, being 40 miles long and 25 miles wide. It is composed of nearly horizontal cretaceous strata, capped everywhere with basalt or andesite, ranging from 200 to 400 feet in thickness. To the northeast and to the south of it are similar high *mesas*, also capped by basalt and andesite, but presenting no great volcanic pile like Mt. Taylor. The only features which indicate volcanic vents are barely noticeable hillocks, which scarcely affect the evenness of the horizontal surfaces and which are wholly incommensurate, apparently, with the vast lava caps upon which they occur.

These lavas are all of tertiary age. It would be difficult to say to what divisions of tertiary time their activity should be assigned, but it cannot have been very late tertiary and it is reasonably certain that it cannot have been very old tertiary. In a general way their activity is inferred to have prevailed in a period not far from middle tertiary time—possibly in the miocene. The large amount of erosion which has occurred since their eruptions ceased forbids a much later period, and the still larger amount of tertiary erosion which preceded this activity equally forbids a much earlier one.

Upon the summits of the *mesas* no recent eruptive rocks occur. But in the broad valleys which lie between them and around them are lavas of quite another age. These valley lavas are all recent. Indeed the most superficial observer is at once impressed with the freshness of their aspect, and critical examination confirms the view that none of them have any geologic antiquity, while some of them are so modern that it seems as if half a dozen centuries were a large estimate of the time which separates us from their outflow.

These recent eruptions are basalts of normal type. The external aspects of the fields of young lava resemble those of the Hawaiian Islands. The two forms of solidified lava are well presented, viz: the viscous or ropy, and the rough clinker fields.

A striking characteristic of both old and young lavas—those upon the *mesa* summits and those in the valleys below—is the usual though not universal absence of cinder cones or piles of fragmental matter built up around the orifices from which the lavas were extruded. The eruptions, with the exception of those of Mt. Taylor, belonged to the quiet order which are typified among volcanoes now active, by Mauna Loa and Kilauea.

But the volcanic remnants which appeal most strongly to the imagination of the observer, remain to be described. In the broad valleys which separate the lava-capped *mesas* are seen many conspicuous objects rising as sharp peaks or aiguilles of rock to great altitudes. They are very black in color, and contrast powerfully with the bright tints of the sedimentary beds around them. These peaks, which range in altitude above the valley plains from 700 or 800 feet to 2,000 feet, consist of columnar basalt. They are, in fact, the ancient lavas which congealed in the volcanic pipes, while the sedimentary strata which formerly inclosed them have been swept away in the great erosion of the country. In that long-continued and great denudation these "necks," by their more adamantine character, have resisted the general decay, and remain to attest the former extension of the strata over the valleys and the existence, prior to their denudation, of volcanic extravasations which probably covered them wholly or in part. In the *mesa* walls and on their slopes may be seen numerous instances of partially excavated necks, while in others the necks are just beginning to be exhumed. In the latter cases remnants of the old cinder-cones which were piled up over their summits are still preserved, so that natural sections of the whole apparatus are exhibited. There are many scores of these necks, and the effects of erosion in unearthing them are exhibited in all stages. Wherever the true neck or core is disclosed the basalt is seen to be columnar, and the columns are often arranged in beautiful fashions.

No more striking illustration and proof of a great erosion could be mentioned than is here disclosed, and the region must become a classic one, to be referred to by future geologists as an excellent example of some of the grandest laws and processes with which their science deals.

Mr. POWELL spoke of the distribution of eruptions. They are apt to occur on the faces of acclivities undergoing erosion, but not on acclivities due to displacement. Near a fault they break through the uplifted block rather than the thrown. They do not occur in the bottoms of cañons.

In mapping the Plateaus he had thrown the boundary farther north than Captain Dutton, so as to include a large area north of the Uinta Mountains.

The peculiarly favorable conditions under which geology is studied in the plateau region enable its features to be comprehended without the doubts and the laborious compilation of details elsewhere necessary. It results that while the structure of the Plateau country is as well known as that of any equal area in the world, the literature of its geology is exceedingly small.

Other remarks were made by Messrs. WHITE and GILBERT.

258TH MEETING. NOVEMBER 22, 1884.

The President in the Chair.

Forty-nine members and guests present.

Mr. E. B. ELLIOTT made a communication on

ELECTRIC LIGHTING,

which was discussed by Messrs. HILGARD, WELLING, MUSSEY, PAUL, and POWELL.

Mr. H. ALLEN HAZEN made a communication on

THERMOMETER EXPOSURE.

[Abstract.]

In recent experiments for determining the relative values of temperatures in city and country, it has been found that ordinarily, on clear days, in the early morning, at 6 feet above ground, in the country, temperatures are 4 to 5 degrees lower than in the city, and also that the air is always nearly saturated in the country, but not as nearly in the city. This is due more to intense radiation from grass in the country, this cooling the air to the dew point, than to the heating and drying from pavements and walls or chimneys of houses.

To obtain a standard air temperature it is proposed to use bright and black bulb thermometers joined together and swung over grass ground under an umbrella, with no shade from trees or buildings, in the day time. Under such circumstances the two thermometers can be brought within 0.5° of each other, and the true air temperature may be taken as about as much lower than the bright-bulb as that is lower than the black.

Recent experiments with six different thermometer shelters indicate a general agreement, except in the case of the Wild shelter. The peculiar condition effected by the Wild shelter is inferior ventilation, and the experiments indicate the practical sufficiency of the single-louvred shelter. To determine the humidity with the psychrometer in still air, the employment of artificial ventilation is recommended.

Remarks were made by Mr. PAUL.

259TH MEETING. DECEMBER 6, 1884.

By courtesy of the officers of the Columbian University, the meeting was held in the lecture hall of the University building.

Members of the Anthropological, Biological, and Chemical Societies and their friends were present by invitation.

Mr. J. W. POWELL, by request of the President, occupied the Chair.

Present, one hundred and four members and guests.

The business of the evening was the presentation of the Annual Address of the President, Mr. J. C. WELLING. In introducing him to the audience, the Chairman sketched the history of the Society, describing the socio-scientific club of which it was the offspring, and referring to the younger scientific societies of Washington, of which it might be regarded as the parent.

The President then read an address on

THE ATOMIC PHILOSOPHY, PHYSICAL AND METAPHYSICAL.

[Printed in full on pp. XXIX–LIX.]

On motion of Mr. GREGORY, the Society tendered its President a vote of thanks for his efficient administration and instructive address.

260TH MEETING. DECEMBER 20, 1884.

THE FOURTEENTH ANNUAL MEETING.

The President in the chair.

The Chair announced the death, since the last meeting, of Mr. HENRY WAYNE BLAIR.

The Chair announced the election to membership of Mr. ROBERT EDWARDS CARTER STEARNS.

It was announced that the Mathematical Section would, in the future, hold its meetings in the mathematical class room of the Columbian University, the use of that room having been tendered by the officers of the University.

The order of business was then read, and afterward the minutes of the last annual meeting.

The report of the Secretaries were read and accepted. (Printed on page XXIII.)

The report of the Treasurer was read, received, and referred to an Auditing Committee, consisting of Messrs. H. C. Yarrow, Marcus Baker, and W. C. Winlock. (The report is printed on pages XXIV and XXV.)

The minutes of the 258th and 259th meetings were read and approved.

The officers of the ensuing year were then elected. (The list is printed on page XV.)

The rough minutes of the meeting were read, and the meeting adjourned.

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON

MATHEMATICAL SECTION.

STANDING RULES

OF THE

MATHEMATICAL SECTION.

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1. The object of this Section is the consideration and discussion of papers relating to pure or applied mathematics.

2. The special officers of the Section shall be a Chairman and a Secretary, who shall be elected at the first meeting of the Section in each year, and discharge the duties usually attaching to those offices.

3. To bring a paper regularly before the Section it must be submitted to the Standing Committee on Communications for the stated meetings of the Society, with the statement that it is for the Mathematical Section.

4. Meetings shall be called by the Standing Committee on Communications whenever the extent or importance of the papers submitted and approved appear to justify it.

5. All members of the Philosophical Society who wish to do so may take part in the meetings of this Section.

6. To every member who shall have notified the Secretary of the General Committee of his desire to receive them, announcements of the meetings of the Section shall be sent by mail.

7. The Section shall have power to adopt such rules of procedure as it may find expedient.

OFFICERS

OF THE

MATHEMATICAL SECTION FOR 1884.

Chairman, ASAPH HALL. *Secretary*, HENRY FARQUHAR.

LIST OF MEMBERS WHO RECEIVE ANNOUNCEMENT OF THE MEETINGS.

ABBE, C.
AVERY, R. S.
BAKER, M.
BATES, H. H.
BILLINGS, J. S.
BURGESS, E. S.
CHRISTIE, A. S.
COFFIN, J. H. C.
CURTIS, G. E.
DELAND, T. L.
DOOLITTLE, M. H.
EASTMAN, J. R.
EIMBECK, W.
ELLIOTT, E. B.
FARQUHAR, H.
FLINT, A. S.
GILBERT, G. K.
GORE, J. H.
GREEN, B. R.
HALL A.
HARKNESS, W.
HAZEN, H. A.
HILGARD, J. E.
HILL, G. W.
KING, A. F. A.
KUMMELL, C. H.
MCGEE, W J
NEWCOMB, S.
PAUL, H. M.
LEFAVOUR, E. B.
PEIRCE, C. S.
RITTER, W. F. M'K.
SMILEY, C. W.
TAYLOR, W. B.
UPTON, W. W.
WALLING, H. F.
WINLOCK, W. C.
WOODWARD, R. S.

BULLETIN

OF THE

MATHEMATICAL SECTION.

10TH MEETING. JANUARY 30, 1884.

The Chairman presided.

Seventeen members and guests present.

The Section proceeded, under Rule 2, to the election of a Chairman and a Secretary for the year 1884. On motion of Mr. ELLIOTT, the rules governing the elections of the Society were adopted. The officers for 1883—Mr. HALL, as Chairman, and Mr. H. FARQUHAR, as Secretary—were re-elected, after each had briefly expressed a desire that the choice might fall on some one else.

Mr. KUMMELL read an extract from a letter lately received from Mr. Artemas Martin, of Erie, Pennsylvania, in which the formation of an American Mathematical Society was recommended. After some informal discussion, Mr. WINLOCK moved the appointment of a special committee, with instructions to report on the advisability of taking steps for the formation of such a society. On motion of Mr. ELLIOTT, the matter was postponed.

Mr. KUMMELL then made a communication on

CURVES SIMILAR TO THEIR EVOLUTES,

in which he made use of the intrinsic equation, and showed this property to belong to a whole class, of which the logarithmic spiral is at one extreme and the cycloids are at the other.*

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* Prof. Benjamin Peirce solved a problem almost identical with this one, in *Gill's Mathematical Miscellany* for May, 1839, by essentially the same methods. This solution, which had not been seen by Mr. Kummell at the time of reading his paper, is believed to contain the first use of what has since become known as the "intrinsic equation."

Remarks on this communication were made by Messrs. CHRISTIE and HILL.

Mr. G. K. GILBERT made a communication on

THE PROBLEM OF THE KNIGHT'S TOUR.

[Abstract.]

The ordinary problem, requiring the knight to traverse the chess-board and return to his original position in sixty-four moves, is susceptible of very numerous solutions, and is not difficult. Its interest is increased by extending it so as to include fields of other form and size.

It is readily shown that a perfect tour is impossible on any field containing an odd number of squares.

A *symmetric* tour is one divisible into two or more similar parts. A tour has *bilateral symmetry* when one-half, being turned face downward upon the other, coincides with it. A tour has *biradial symmetry* when one-half, being rotated through 180° about the center of figure, coincides with the other half. A tour has *quadriradial symmetry* when its fourth part, being rotated through 90° about the center of figure, coincides with the adjacent quarter.

A tour having bilateral symmetry cannot be devised on a field containing a number of squares divisible by four.

A tour having biradial symmetry cannot be devised on a field whose number of squares is divisible by two and not by four.

A tour having quadriradial symmetry cannot be devised on a field whose number of squares is divisible by eight.

It follows that *on square fields* the tour is impossible if the number of spots on a side is odd; bilateral symmetry is never possible; quadri-radial symmetry is possible only when the number of squares on a side is the double of an odd number. The only symmetry possible on a chess-board is biradial.

The above conclusions are deductive. It is determined empirically that the smallest square field on which the tour can be executed is that with 36 spots. Upon this field the number of possible tours with biradial symmetry is twenty-one, of which five have also quadriradial symmetry.

Remarks on this communication were made by Messrs. ELLIOTT and HALL, who called attention to previous work on the subject.

11TH MEETING. FEBRUARY 20, 1884.

The Chairman presided.

Eighteen members and guests present.

Mr. H. FARQUHAR made a communication on

EMPIRICAL FORMULÆ FOR THE DIMINUTION OF AMPLITUDE OF A FREELY-OSCILLATING PENDULUM.

[Abstract.]

The theoretical formulæ usually employed are obtained by integration from an expression for the diminution of the amplitude in terms of the amplitude itself. The most important term in this expression is one involving the first power of the amplitude, indicating a resistance proportional to the velocity of the pendulum's motion. A term containing the square of the velocity (or amplitude) also enters; and, to allow for the friction of the pendulum knife-edge on its support, a term independent of the velocity would have to be added. Atmospheric resistance to very high velocities is found, moreover, to be proportional to a higher power than the square of the velocity. There are thus more than three terms theoretically required to express the resistance, and these must be calculated, such is the uncertainty of the subject and the complexity of the conditions on which the different resistances depend, from the observations themselves. Since these observations must also be depended on for an additional constant (the amplitude at some initial time or the time of some standard amplitude), and since they are not complete or exact enough to furnish more than three constants, or four in a few exceptional cases, it is obvious that a good approximation to theory must content us in practice.

Two convenient methods of representing amplitude in terms of time are suggested by imposing arbitrary conditions. First, taking three terms to express the diminution (the amplitude being φ), thus:

$$a + b\varphi + c\varphi^2,$$

suppose the square of half the middle co-efficient equal to the product of the other two. This expression has then the form:

$$\frac{1}{a}(\varphi + b)^2.$$

Integrating this value of $-D_t\varphi$, and supplying a constant, we have:

$$(\varphi + b)(t - e) = a,$$

in which the constants $a + be$, e and $-b$, are easy to calculate by least squares.

To show the agreement of this formula with observation, take Mr. Pierce's "mean swing" at three European stations (U. S. Coast Survey Report for 1876, appendix 15, pages 232, 271) and apply $b = 29'.2$, $e = -7632^s$, $a = 756847$, in calculating φ from t. Hence the following table:

t.	φ, obs'd.	φ, calc'd.	Residuals (1st).	Residuals (2d).
-2880^s	130′	130′.07	−0′.07	−0′.16
−2187	110	109.80	+0.20	+0.13
−1779	100	100.11	−0.11	−0.04
−706	80	80.08	−0.08	+0.24
0	70	69.97	+0.03	+0.40
+1927	50	49.98	+0.02	0.00
+3304	40	40.01	−0.01	−0.66

The agreement (in column "residuals, 1st") is as close as could be desired. The equation is that of the equilateral hyperbola, with asymptotes parallel to the axes of φ and t. This agreement can be made still closer by inclining one of the asymptotes, a term $-c\,(t - e)^2$ being added. There are thus four constants to compute; but this form of equation has the advantage of having its constants directly deducible by least square reduction. With the additional term, a perfect agreement between theory and the most precise observations hitherto made can be attained. As an instance, the thirty-five observations of amplitude, from over 2° down to 10′, given by Prof. Oppolzer in the Proceedings of the Vienna Academy for October, 1882, were compared with the formula

$$(\varphi + 60'.6)\,(t + 10.8) - 0.5\,(t + 10.8)^2 = 2178.1$$

(the unit of t being an interval of about $5^m.7$) and of the residuals, which need not be given in detail, the largest was 0′.8. A similar accordance was found in a set of observations extending over six hours, the pendulum swinging under less than half an inch of atmospheric pressure. (See Mr. Pierce's report, page 248, last two columns combined.) In this formula,

$$D_t\varphi = -\frac{1}{2a}\left\{(\varphi+b)^2 - 4ac + (\varphi+b)\sqrt{(\varphi+b)^2-4ac}\right\}$$

$$= -\frac{1}{a}(\varphi+b)^2 + 3c + \frac{ac^2}{(\varphi+b)^2} + \text{etc.}$$

The correction to the time of oscillation $\left(\frac{1}{16} D_t^{-1} \varphi^2\right)$ involves the logarithm of $t - e$, and is not very simple in practical application.

The second convenient method is the one by which the residuals in the last column of the table above given were calculated. In this the rate of diminution is supposed proportional to φ^{1+n}, n being a proper fraction. Hence,

$$\varphi^n(t-e) = a, \text{ and } D_t^{-1}\varphi^2 = -\frac{na^{\frac{2}{n}}}{(2-n)(t-e)^{\frac{2}{n}-1}} = -\frac{na\varphi^{2-n}}{2-n}$$

This formula is very simple, and the table shows its agreement with observation to be fair for the larger amplitudes—those of chief importance. In this calculation $n = \frac{1}{2}$, $e = -10716^s$, and $a = 89400$. Better results would have been obtained by using a slightly smaller value of n, say 0.44; but in practice the nearest tenth or reciprocal of a whole number is sufficient. In reducing the observations given by Prof. Oppolzer, n was taken equal to 0.28; but one of the residuals exceeded 1′, though two others were as high as 0′.9. The observations at low pressures, above referred to, indicated a much smaller n. By using the value 0.04, however, the agreement of formula and observation was perfect. n thus appears to be nearly proportional to the square root of the atmospheric pressure; but when very small, it may be supposed to vanish, and φ^n replaced by the logarithm of φ. In this case e will of course be the time of unit-amplitude, instead of that of infinite amplitude as in former cases.

No two observations of the diminution of amplitude of the same pendulum will in general be found to be copies of each other, for differences in atmospheric conditions and in friction on the support, imperceptible otherwise, will manifest themselves in a changed rate of diminution. Even in calculating the correction for different parts of one extended swing, it is advisable to adopt different values of one or other of the constants found. By so varying the quantity *e*, in the formula last given, all disadvantages from its want of exact accordance with observation disappear, and the results are brought far within the needful limits of accuracy.

Mr. GILBERT then stated

A CONCRETE PROBLEM IN HYDROSTATICS,

suggested by the fact that the shore-line of a quaternary lake in the Great Basin is shown by levels to be more than a hundred feet higher on elevated land, that once formed islands near its middle part, than on the margin of the lake. This inland sea, known as Lake Bonneville, was one hundred and twenty miles across. Among the possible explanations of the present difference of level, the effect of the removal of a large body of water in changing the form of level surfaces in its basin had been suggested, and the problem was to find how great an effect was due to this cause.

In the discussion that followed, Mr. PAUL called attention to the complexity of the calculation of equipotential surfaces.

Mr. WOODWARD had formerly made a somewhat similar computation to ascertain the deflection of the plumb-line caused by unequal local attraction to eastward and to westward at the eastern end of Lake Ontario; from which it appeared to result that the effect due to this cause was insignificant in comparison with that required by the problem.

Other remarks were made by Messrs. DOOLITTLE, HILL, H. FARQUHAR, and S. J. BROWN.

At the request of the Chairman, a communication promised by him was postponed until next meeting.

12TH MEETING. MARCH 5, 1884.

The Chairman presided.

Fifteen members present.

Mr. A. HALL read the following paper on

THE FORMULÆ FOR COMPUTING THE POSITION OF A SATELLITE.

The method of rectangular co-ordinates in space furnishes a very simple and at the same time a general method of treating many questions in astronomy. This method was introduced into practical astronomy by Lagrange in his memoir on the Transit of Venus, June 3, 1769 (Berlin Academy Memoirs, 1766). Whenever we have to consider the relations of three points in space, we may take the origin of co-ordinates at one of the points, and then forming the values of the rectangular co-ordinates of the other points in terms of the polar co-ordinates, the sum or difference of two of the x co-ordinates being equal to the third x co-ordinate, we have an equation between the three polar co-ordinates. Similar relations hold for the axes of y and z, and hence result three equations between the two angles and the distance that are required to be found. This method is extremely useful, and can be applied to a great number of questions in parallax, aberration, eclipses, and to those that occur in nearly every part of spherical astronomy. A great recommendation of this method is its simplicity, and the fact that it is so closely connected with first principles that it can be applied with the greatest ease. After the equations are formed they have only to be transformed by known rules, and the whole work is thus reduced to algebraic and trigonometric transformations which can be safely made. These advantages are so great that it is not surprising that this method of treating astronomical questions has come so largely into use, and the generality and elegance of the process are in marked contrast with the old methods which proceed by spherical trigonometry. Perhaps a disadvantage of the new method is that it is too mechanical, and one is apt to forget or never know the meaning of the quantities that are employed. The old geometrical methods have therefore their value in calling to mind a more exact knowledge of the quantities that are used in the solution of a problem.

In the method which Bessel has employed for computing the position of a satellite, he has derived his formulæ by Lagrange's method. Thus if α and δ be the apparent right ascension and declination of the planet at any instant, α', δ' the same quantities for the satellite, and if ρ and ρ' be their distances from the earth, and if r be the radius vector of the satellite, and a and d its right ascension and declination seen from the planet, we have, by the method of rectangular co-ordinates,

$$\begin{aligned} \rho' \cos\delta' \cos\alpha' &= \rho \cos\delta \cos\alpha + r \cos d \cos a \\ \rho' \cos\delta' \sin\alpha' &= \rho \cos\delta \sin\alpha + r \cos d \sin a \\ \rho' \sin\delta' &= \rho \sin\delta + r \sin d \end{aligned} \qquad (1)$$

If p and s are the angle of position and distance of the satellite with respect to the center of the planet, the spherical triangle formed by the pole of the equator, the planet, and the satellite gives us the following equations:

$$\begin{aligned} \cos s &= \sin\delta \sin\delta' + \cos\delta \cos\delta' \cos(\alpha' - \alpha) \\ \sin s \cos p &= \cos\delta \sin\delta' - \sin\delta \cos\delta' \cos(\alpha' - \alpha) \\ \sin s \sin p &= \cos\delta' \sin(\alpha' - \alpha) \end{aligned} \qquad (2)$$

If N and J be the longitude of the node of the orbit of the satellite on the equator, and its inclination to the equator, and u the distance of the satellite from the node counted on its orbit, we have

$$\begin{aligned} \cos d \sin(a - N) &= \sin u \cos J \\ \cos d \cos(a - N) &= \cos u \\ \sin d &= \sin u \sin J \end{aligned} \qquad (3)$$

These three sets of equations are fundamental, and are sufficient for the complete solution of the problem—Given the orbit of a satellite to determine its apparent angle of position and distance. We have only to transform these equations, and, in order to ease the computation, to introduce, as Bessel has done, certain auxiliary quantities which depend on the position of the planet in the heavens, and the position of the orbit of the satellite with respect to the equator. These auxiliary quantities will of course vary with the position of the planet, and also from the slow changes that the node and inclination of the orbit undergo, but they can be tabulated easily. So far therefore, as the practical solution of this question is concerned there is not much more to be desired, but it is interesting to look at the problem from another point of view, and one that will lead us to consider more closely its geometry.

Imagine a set of rectangular axes in space, the origin being at the center of the planet, and denote by X, Y, Z the points on the celestial sphere made by the intersections of these axes. Let S be the point where the prolongation of the radius vector of the satellite strikes the sphere; then we have for the co-ordinates of the satellite

$$x = r.\cos SX$$
$$y = r.\cos SY$$
$$z = r.\cos SZ$$

We can express these cosines by means of six auxiliary quantities similar to those that Gauss has used for computing the position of a planet. Take the prolongation of the right line drawn from the earth to the planet as the axis of Z, the axis of Y in the plane of the declination circle that passes through Z, and the axis of X at right angles to this plane and in the direction of increasing right ascensions. Let O be the pole of the equator and T the positive pole of the orbit of the satellite. Introduce the following notation, which is the same as Bessel's:

$$\text{arc } TX = f, \quad \text{angle } OTX = F$$
$$\text{" } TY = g, \quad \text{" } OTY = G$$
$$\text{" } TZ = h, \quad \text{" } OTZ = H$$

Since the arc $TS = 90°$, the spherical triangles STX, STY, and STZ give

$$\cos SX = \sin f \cos STX$$
$$\cos SY = \sin g \cos STY$$
$$\cos SZ = \sin h \cos STZ$$

The distance of the satellite in its orbit from the node being u, and the angle OTN being 90°, we have

$$STX = 90° - (F + u)$$
$$STY = 90° - (G + u)$$
$$STZ = 90° - (H + u)$$

And the values of the co-ordinates are therefore:

$$x = r.\sin f \sin (F + u)$$
$$y = r.\sin g \sin (G + u) \qquad (4)$$
$$z = r.\sin h \sin (H + u)$$

These are the values at which Bessel arrives by the analytical method. The arcs f, g, h are always less than 180°, and the only difficulty is in counting the angles F, G, H. In the purely analytical process we merely substitute so as to satisfy the equations, and the result is right if we pay attention to the algebraic signs; but in the preceding quasi geometrical method we must be careful to count the angles F, G, H in the direction of increasing right ascensions from 0° to 360°. The formulæ for computing the six auxiliary quantities can be found from the spherical triangles TOX, TOY, TOZ. In these triangles the angles at O are

$$TOX = 180° - (\alpha - N)$$
$$TOY = 90 - (\alpha - N)$$
$$TOZ = 90 + (\alpha - N)$$

Hence, we have

$$\begin{aligned}
\cos f &= -\sin J \cos(\alpha - N) \\
\sin f \sin F &= -\sin(\alpha - N) \\
\sin f \cos F &= \cos J \cos(\alpha - N) \\
\cos g &= \cos\delta \cos J + \sin\delta \sin J \sin(\alpha - N) \\
\sin g \sin G &= -\sin\delta \cos(\alpha - N) \qquad (5) \\
\sin g \cos G &= \cos\delta \sin J - \sin\delta \cos J \sin(\alpha - N) \\
\cos h &= \sin\delta \cos J - \cos\delta \sin J \sin(\alpha - N) \\
\sin h \sin H &= \cos\delta \cos(\alpha - N) \\
\sin h \cos H &= \sin\delta \sin J + \cos\delta \cos J \sin(\alpha - N)
\end{aligned}$$

The computation of these formulæ may be changed by introducing other auxiliary quantities, as is commonly done, but nothing is gained by such a change if the computer is accustomed to the use of addition and subtraction logarithms.

By means of the spherical triangles we can find a number of elegant relations among the quantities f, g, h, F, G, H. But we have first

$$\cos f^2 + \cos g^2 + \cos h^2 = 1,$$

or these are the direction cosines of the line drawn from the planet to the pole of the orbit of the satellite.

The triangle XTY gives

$$\cos XY = \cos XT \cos YT + \sin XT \sin YT \cos XTY,$$

and we have $\quad XY = 90°, XTY = F - G,$

hence the values of cos XY, cos YZ, cos ZX furnish the equations

$$\begin{aligned} \cos(F - G) &= -\operatorname{cotg} f \operatorname{cotg} g \\ \cos(G - H) &= -\operatorname{cotg} g \operatorname{cotg} h \\ \cos(H - F) &= -\operatorname{cotg} h \operatorname{cotg} f \end{aligned} \qquad (6)$$

Again the triangle XTY gives

$$\cos f = \sin g \cos TYX,$$

and from the triangle TYX

$$\sin h \sin YTZ = \sin TYZ,$$

but $$TYX - TYZ = 90°,$$

and $$YTZ = -(G - H),$$

hence these equations and similar ones give

$$\begin{aligned} \sin(F - G) &= \frac{\cos h}{\sin f \sin g} \\ \sin(G - H) &= \frac{\cos f}{\sin g \sin h} \\ \sin(H - F) &= \frac{\cos g}{\sin h \sin f} \end{aligned} \qquad (7)$$

By combining equations (6) and (7), we have

$$\operatorname{cotang}(F - G) = -\frac{\cos f \cos g}{\cos h}$$

$$\operatorname{cotang}(G - H) = -\frac{\cos g \cos h}{\cos f}$$

$$\operatorname{cotang}(H - F) = -\frac{\cos h \cos f}{\cos g}$$

$$\cos f^2 = \operatorname{cotg}(F - G) \operatorname{cotg}(H - F)$$

$$\cos g^2 = \operatorname{cotg}(G - H) \operatorname{cotg}(F - G)$$

$$\cos h^2 = \operatorname{cotg}(H - F) \operatorname{cotg}(G - H)$$

$$\sin f^2 = -\frac{\cos(G - H)}{\sin(F - G)\sin(H - F)}$$

$$\sin g^2 = -\frac{\cos(H - F)}{\sin(G - H)\sin(F - G)}$$

$$\sin h^2 = -\frac{\cos(F - G)}{\sin(H - F)\sin(G - H)}$$

These six auxiliary quantities are therefore strictly analogous to those which Gauss introduced for computing the position of a planet. For controlling the computation, we have

$$\tang J = \frac{\sin g \sin h \sin (H - G)}{\sin f \cos F},$$

an equation in which each of the six auxiliaries enters into the value of J.

If we introduce another auxiliary quantity, and put the angle

$$TZO = 180^\circ - k,$$

it follows, from the manner adopted for counting an angle of position, that

$$TZO = 180^\circ - (p - k).$$

Denoting the angle between the radius vector and the axis of Z by σ, the spherical triangle TZS gives

$$\begin{aligned} \sin \sigma \sin (p - k) &= \cos (H + u) \\ \sin \sigma \cos (p - k) &= \sin (H + u) \cos h \\ \cos \sigma &= \sin (H + u) \sin h \end{aligned} \tag{8}$$

But we have also

$$\begin{aligned} \rho' \sin s &= r \sin \sigma \\ \rho' \cos s &= r \cos \sigma + \rho, \end{aligned}$$

and by uniting these equations with (8), we can find s and p. This method of finding the distance and the angle of position is due to Marth, and as it is in constant use by him for the very convenient ephemerides of satellites which he publishes, it may be well to consider it further. If we multiply equations (8) by r, and then substitute the values of $r \sin \sigma$ and $r \cos \sigma$ from the last equations, we have

$$\begin{aligned} \rho' \sin s \sin (p - k) &= r \cos (H + u) \\ \rho' \sin s \cos (p - k) &= r \sin (H + u) \cos h \\ \rho' \cos s &= r \sin (H + u) \sin h + \rho \end{aligned} \tag{9}$$

Instead of these exact equations we may use in nearly all known cases of satellites the first two equations and put ρ for ρ' and s for $\sin s$. The equations for use are then

$$\begin{aligned} s \sin (p - k) &= \frac{r}{\rho} \cos (H + u) \\ s \cos (p - k) &= \frac{r}{\rho} \sin (H + u) \cos h \end{aligned} \tag{10}$$

If we express s and r in seconds of arc, and assume that the orbit is circular, $\frac{r}{\rho}$ will be the semi-major axis of the apparent ellipse described by the satellite, and $\frac{r}{\rho}\cos h$ will be the semi-minor axis. The quantities $\frac{r}{\rho}$, $\frac{r}{\rho}\cos h$, H and k can be tabulated, and equations (10) furnish the easy method of computing s and p which is employed by Marth (Monthly Notices, Royal Astronomical Society.)

For computing k we have from the triangle TZO

$$\sin h \sin k = \cos(\alpha - N)\sin J$$
$$\sin h \cos k = -\sin(\alpha - N)\sin J \sin\delta - \cos J\cos\delta \quad (11)$$

and, also,

$$\sin h \sin k = -\cos f$$
$$\sin h \cos k = -\cos g$$

In what precedes it is assumed that the orbit of the satellite is known. If this orbit is not known the easiest method of proceeding seems to be the following: First, we assume the orbit of the satellite to be a circle, and from the observed angles of position and the observed distances determine the major and minor axes of the apparent ellipse described by the satellite around the planet, and the angle of position of the minor axis. Generally these quantities can be found by a graphical method. The preceding angle k is the angle of position of the minor axis, and $\cos h$ is found from the ratio of the two axes. Then from the triangle TOZ we have the equations

$$\sin J\cos(N - \alpha) = \sin h \sin k$$
$$\sin J \sin(N - \alpha) = \cos h\cos\delta + \sin h\sin\delta\cos k \quad (12)$$
$$\cos J = \cos h\sin\delta - \sin h\cos\delta\cos k$$

With the approximate values of J and N found from these equations we can compute the auxiliary quantities depending on the position of the plane of the orbit and the position of the planet, and can determine the elements belonging to the plane of the orbit. These approximate elements can afterwards be corrected by equations of condition or by other methods.

In work of this kind it is more convenient to have the inclination and node of the orbit referred to the equator, and since these elements are commonly given with respect to the ecliptic we have to transfer them to the equator. If n and i are the node and inclination referred to the ecliptic, ε the obliquity of the equator, and w the distance from the ecliptic to the equator counted on the orbit. we have the following equations for finding J, N, and w. These equations come from the triangle between the equator, the ecliptic, and the orbit of the satellite. They are similar to those given in the Theoria Mot., Art. 55,

$$\sin \tfrac{1}{2} J \cos \frac{w - N}{2} = \cos \frac{n}{2} \sin \frac{\varepsilon + i}{2}$$

$$\sin \tfrac{1}{2} J \sin \frac{w - N}{2} = \sin \frac{n}{2} \sin \frac{\varepsilon - i}{2}$$

$$\cos \tfrac{1}{2} J \cos \frac{w + N}{2} = \cos \frac{n}{2} \cos \frac{\varepsilon + i}{2}$$

$$\cos \tfrac{1}{2} J \sin \frac{w + N}{2} = \sin \frac{n}{2} \cos \frac{\varepsilon - i}{2}$$

For the inverse problem of finding i, N, and w from J, N, and ε, we have from the same triangle

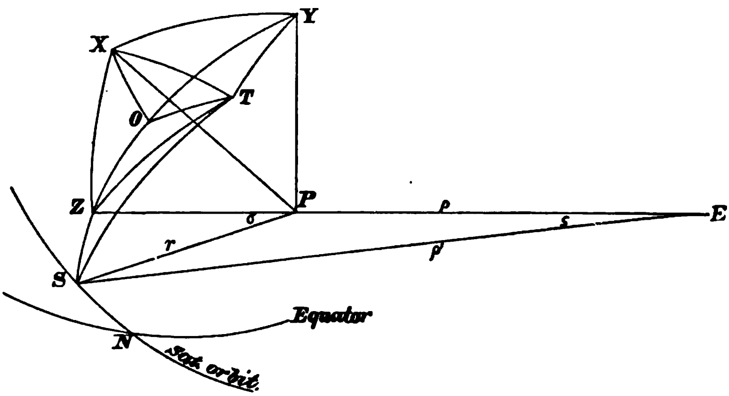
$$\cos \tfrac{1}{2} i \cos \frac{n - w}{2} = \cos \frac{N}{2} \cos \frac{J - \varepsilon}{2}$$

$$\cos \tfrac{1}{2} i \sin \frac{n - w}{2} = \sin \frac{N}{2} \cos \frac{J + \varepsilon}{2}$$

$$\sin \tfrac{1}{2} i \cos \frac{n + w}{2} = \cos \frac{N}{2} \sin \frac{J - \varepsilon}{2}$$

$$\sin \tfrac{1}{2} i \sin \frac{n + w}{2} = \sin \frac{N}{2} \sin \frac{J + \varepsilon}{2}$$

POSITION OF A SATELLITE.



$TX = f,\ OTX = F,\ OT = J,\qquad STZ = 90^\circ - (H + u)$
$TY = g,\ OTY = G,\ OY = \delta,\qquad TZO = 180^\circ - k$
$TZ = h,\ OTZ = H,\ OZ = 90^\circ - \delta,\ TZS = 180^\circ - (p - k)$
$NS = u,\ NOZ = a - N,\ TOZ = 90^\circ + (a - N),\ SZO = 360^\circ - p$

$$TOX = TOY + 90 = 180^\circ - (a - N)$$

N is the pole of OT, $\therefore NOT = NTO = 90^\circ$

In response to a question, Mr. HALL said that in computations of orbits of double stars, as little reliance should be placed upon measures of distance as possible. Variations of angular velocity are far safer.

Mr. G. W. HILL made a communication on

A FORMULA FOR THE LENGTH OF A SECONDS-PENDULUM,

which is published in full in the Astronomical Papers of the American Ephemeris, Vol. III, Part 2, Chapter V.

13TH MEETING. MARCH 26, 1884.

The Chairman presided.

Fourteen members present.

Mr. ALEX. S. CHRISTIE made a communication on

A FORM OF THE MULTINOMIAL THEOREM.

This communication is reserved by the author. Remarks were made by Mr. HILL.

Mr. R. S. WOODWARD gave a

DISCUSSION OF A CONCRETE PROBLEM IN HYDROSTATICS PROPOSED BY MR. G. K. GILBERT.

Remarks on this communication were made by Mr. GILBERT.

Mr. C. H. KUMMELL gave the first part of a communication on

THE QUADRIC TRANSFORMATION OF ELLIPTIC INTEGRALS,

which was unfinished when the hour of adjournment arrived.

14TH MEETING. MAY 7, 1884.

The Chairman presided.

Nine members present.

In the absence of the Secretary, the minutes were read by Mr. CHRISTIE.

Mr. KUMMELL finished the paper begun by him at last meeting on

THE QUADRIC TRANSFORMATION OF ELLIPTIC INTEGRALS, COMBINED WITH THE ALGORITHM OF THE ARITHMETICO-GEOMETRIC MEAN.

[Abstract.]

The algorithm of the arithmetico-geometric mean, so remarkable for its symmetry and convenience, was first used by Gauss many years before the brilliant era of Abel and Jacobi. The form which the theory of elliptic functions assumed under the hands of these eminent geometers, though extremely beautiful, might be improved from a practical point of view by a combination with the Gaussian algorithm. In the attempt to do this, the defects of the usual notation became very annoying, and gradually the new, simple, and consistent system of notations, as used in the following, resulted:

I assume for the type of an integral of the first species,

$$u = \int_0^\varphi \frac{a\,d\varphi}{\sqrt{a^2 - c^2 \sin^2\varphi}} = \int_0^\varphi \frac{a\,d\varphi}{\sqrt{a^2 \cos^2\varphi + b^2 \sin^2\varphi}}$$

$$= \int_0^\varphi \frac{d\varphi}{\sqrt{1 - \gamma^2 \sin^2\varphi}} = \int_0^\varphi \frac{d\varphi}{\sqrt{\cos^2\varphi + \beta^2 \sin^2\varphi}} = \varphi\gamma \qquad (1)$$

For the inverse of this I write $u_{-\gamma} = \varphi$. (2)

By (1) we have the modulus $\gamma = \frac{c}{a}$ and the complementary modulus $\beta = \frac{b}{a}$. The letters γ and β are used throughout as symbols for $\frac{c}{a}$ and $\frac{b}{a}$, respectively, and are expressed in a, b, and c whenever required.

In the theory of elliptic functions, sin am$u$, cos am$u$, Δ am$u$ (Jacobi's notation) or sn$u$, cn$u$, dn$u$ (Gudermann's notation), the elliptic quadrant K (Jacobi) is the numerical unit of their period. Consistency requires the use of the quadrant as a unit for trigonometric functions also. Let $\lrcorner$ denote a circular quadrant (ordinarily denoted $\frac{\pi}{2}$); then we have, by the notation just explained,

$$\int_0^{\lrcorner} \frac{d\varphi}{\sqrt{1-\gamma^2 \sin^2\varphi}} = \lrcorner_\gamma \text{ (} = K \text{ of Jacobi).} \qquad (3)$$

The complementary integral then

$$\int_0^{\lrcorner} \frac{d\varphi}{\sqrt{1-\beta^2 \sin^2\varphi}} = \lrcorner_\beta \text{ (} = K' \text{ of Jacobi).} \qquad (4)$$

If n is an integer, then, and only then, $(n\,\lrcorner)_\gamma = n\,\lrcorner_\gamma$. (5)

Thus we should be careful in distinguishing between integrals such as

$$(\tfrac{1}{2}\lrcorner)_\gamma = \int_0^{\frac{1}{2}\lrcorner} \frac{d\varphi}{\sqrt{1-\gamma^2 \sin^2\varphi}} \text{ and } \tfrac{1}{2}\,\lrcorner_\gamma = \tfrac{1}{2}\int_0^{\lrcorner} \frac{d\varphi}{\sqrt{1-\gamma^2 \sin^2\varphi}}$$

According to the system of notation just explained, it is unnecessary to use the Jacobian am or the Gudermannian n, neither of which define the functional relation completely, and we write simply

$$\sin\varphi = \sin u_{\lrcorner\gamma} \text{ (} = \sin \text{am}u \text{ of Jacobi or sn}u \text{ of Gudermann)}$$

$$\cos\varphi = \cos u_{\lrcorner\gamma} \text{ (} = \cos \text{am}u \text{ of Jacobi or cn}u \text{ of Gudermann)}$$

$$\sqrt{1-\gamma^2\sin^2\varphi} = \Delta\varphi = \Delta u_{\lrcorner\gamma} \text{ (} = \Delta\,\text{am}u \text{ of Jacobi or dn}u \text{ of Gudermann)} \qquad (6)$$

I remark that none of the usual notations indicate the modulus, and a grave objection to Gudermann's is that it is apt to give the impression that sn$u$ and cn$u$ are not an ordinary sine and cosine. I shall now give in this notation a number of well-known relations, of which use will be made hereafter. The theorem of addition is, if u and v are two integrals to the modulus γ,

$$\sin (u \pm v)_{-\gamma} = \sin u_{-\gamma} \cos v_{-\gamma} \Delta\, v_{-\gamma} \pm \sin v_{-\gamma} \cos u_{-\gamma} \Delta\, u_{-\gamma} \div 1 - \gamma^2 \sin {}^2 u_{-\gamma} \sin v_{-\gamma}$$

$$\cos (u \pm v)_{-\gamma} = \cos u_{-\gamma} \cos v_{-\gamma} \mp \sin u_{-\gamma} \Delta\, u_{-\gamma} \sin v_{-\gamma} \Delta\, v_{-\gamma} \div 1 - \gamma^2 \sin {}^2 u_{-\gamma} \sin v_{-\gamma}$$

$$\Delta\, (u \pm v)_{-\gamma} = \Delta\, u_{-\gamma} \Delta\, v_{-\gamma} \mp \gamma^2 \sin u_{-\gamma} \cos u_{-\gamma} \sin v_{-\gamma} \cos v_{-\gamma} \div 1 - \gamma^2 \sin {}^2 u_{-\gamma} \sin v_{-\gamma} \quad (7)$$

We have

$$\sin (\pm \rfloor) = \pm 1$$
$$\cos (\pm \rfloor) = 0$$
$$\Delta\, (\pm \rfloor) = \beta \quad (8)$$

therefore, replacing v by $\rfloor\gamma$, we have

$$\sin (u \pm \rfloor\gamma) = \pm \frac{\cos u_{-\gamma}}{\Delta\, u_{-\gamma}}$$
$$\cos (u \pm \rfloor\gamma) = \pm \beta \frac{\sin u_{-\gamma}}{\Delta\, u_{-\gamma}}$$
$$\Delta\, (u \pm \rfloor\gamma) = \frac{\beta}{\Delta\, u_{-\gamma}} \quad (9)$$

Replacing in these u by $u \pm \rfloor\gamma$, we have

$$\sin (u \pm 2 \rfloor\gamma) = - \sin u_{-\gamma}$$
$$\cos (u \pm 2 \rfloor\gamma) = - \cos u_{-\gamma}$$
$$\Delta\, (u \pm 2 \rfloor\gamma) = \Delta\, u_{-\gamma} \quad (10)$$

It follows, replacing in these u by $u + 2 \rfloor\gamma$, that $4 \rfloor\gamma$ is the complete period of the elliptic sine and cosine and $2 \rfloor\gamma$ that of the delta.

Placing $u = v$, we have the duplication formulæ:

$$\sin (2u)_{-\gamma} = 2 \sin u_{-\gamma} \cos u_{-\gamma} \Delta\, u_{-\gamma} \div 1 - \gamma^2 \sin {}^4 u_{-\gamma}$$
$$\cos (2u)_{-\gamma} = \cos {}^2 u_{-\gamma} - \sin {}^2 u_{-\gamma} \Delta\, {}^2 u_{-\gamma} \div 1 - \gamma^2 \sin {}^4 u_{-\gamma}$$
$$\Delta\, (2u)_{-\gamma} = \Delta\, {}^2 u_{-\gamma} - \gamma^2 \sin {}^2 u_{-\gamma} \cos {}^2 u_{-\gamma} \div 1 - \gamma^2 \sin {}^4 u_{-\gamma} \quad (11)$$

Replacing in these u by $\frac{1}{2} u$ and solving, we have the dimidiation formulæ:

$$\sin^2 \left(\frac{u}{2}\right)_{-\gamma} = 1 - \cos u_{-\gamma} \div 1 + \Delta\, u_{-\gamma}$$
$$\cos^2 \left(\frac{u}{2}\right)_{-\gamma} = \Delta\, u_{-\gamma} + \cos u_{-\gamma} \div 1 + \Delta\, u_{-\gamma}$$
$$\Delta^2 \left(\frac{u}{2}\right)_{-\gamma} = \beta^2 + \Delta\, u_{-\gamma} + \gamma^2 \cos u_{-\gamma} \div 1 + \Delta\, u_{-\gamma} \quad (12)$$

Jacobi's imaginary transformation consists in assuming

$$\sin \varphi = i \tan \psi$$

or
$$\cos \varphi = \frac{1}{\cos \psi}$$

or
$$\Delta \varphi = \frac{1}{\cos \psi} \sqrt{1 - \beta^2 \sin^2 \psi} = \frac{1}{\cos \varphi} \Delta (\psi \beta)_{-\gamma} \qquad (13)$$

then
$$u = \int_0^{\varphi} \frac{d\varphi}{\Delta\varphi} = i \int_0^{\psi} \frac{d\psi}{\sqrt{1-\beta^2\sin^2\psi}}$$

or
$$u = \varphi\gamma = i\,\psi\beta \qquad (14)$$

therefore, by (13),

$$\sin u_{-\gamma} = i \tan \left(\frac{u}{i}\right)_{-\beta} = \frac{1}{i} \tan (ui)_{-\beta}$$

$$\cos u_{-\gamma} = \frac{1}{\cos \left(\frac{u}{i}\right)_{-\beta}} = \frac{1}{\cos (ui)_{-\beta}}$$

$$\Delta\, u_{-\gamma} = \frac{1}{\cos \left(\frac{u}{i}\right)_{-\beta}} \Delta \left(\frac{u}{i}\right)_{-\beta} = \frac{1}{\cos (ui)_{-\beta}} \Delta\, (ui)_{-\beta} \qquad (15)$$

Using these relations in (7), we obtain the following formulæ for elliptic functions, with complex arguments and complementary moduli:

$$\sin (u \pm vi)_{-\gamma} = \sin u_{-\gamma} \;\; \Delta\, v_{-\beta} \pm i \cos u_{-\gamma} \;\; \Delta\, u_{-\gamma} \sin v_{-\beta} \cos v_{-\beta} \div 1 - \Delta^2 u_{-\gamma} \sin^2 v_{-\beta}$$

$$\cos (u \pm vi)_{-\gamma} = \cos u_{-\gamma} \cos v_{-\beta} \mp i \sin u_{-\gamma} \;\; \Delta\, u_{-\gamma} \sin v_{-\beta} \;\; \Delta\, v_{-\beta} \div 1 - \Delta^2 u_{-\gamma} \sin^2 v_{-\beta}$$

$$\Delta\, (u \pm vi)_{-\gamma} = \Delta\, u_{-\gamma} \cos v_{-\beta}\, \Delta\, v_{-\beta} \mp \gamma^2 i \sin u_{-\gamma} \cos u_{-\gamma} \sin v_{-\beta} \div 1 - \Delta^2 u_{-\gamma} \sin^2 v_{-\beta} \qquad (16)$$

We have
$$\sin (\underline{\rfloor}\beta)_{-\beta} = 1$$
$$\cos (\underline{\rfloor}\beta)_{-\beta} = 0$$
$$\Delta\, (\underline{\rfloor}\beta)_{-\beta} = \gamma \qquad (17)$$

therefore, replacing in (16) v by $\rfloor\beta$, we have

$$\sin\,(u \pm \rfloor\beta\, i)_{-\beta} = \frac{1}{\gamma \sin u_{-\gamma}}$$

$$\cos\,(u \pm \rfloor\beta\, i)_{-\beta} = \mp\, i\, \frac{\Delta\, u_{-\gamma}}{\gamma \sin u_{-\gamma}}$$

$$\Delta\,(u \pm \rfloor\beta\, i)_{-\beta} = \mp\, i \cot u_{-\gamma} \tag{18}$$

Placing in these $u \pm \rfloor\beta\, i$ for u, we have

$$\sin\,(u \pm 2\rfloor\beta\, i)_{-\gamma} = \sin u_{-\gamma}$$

$$\cos\,(u \pm 2\rfloor\beta\, i)_{-\gamma} = -\cos u_{-\gamma}$$

$$\Delta\,(u \pm 2\rfloor\beta\, i)_{-\gamma} = -\,\Delta\, u_{-\gamma} \tag{19}$$

It follows, replacing in these u by $u \pm 2\rfloor\beta\, i$, that $4\rfloor\beta\, i$ is the imaginary period of the elliptic cosine and delta and $2\rfloor\beta\, i$ that of the sine. We have then, if m and μ are integers,

$$\sin\,(u + 4\,m\rfloor\gamma + 2\,\mu\rfloor\beta\, i)_{-\gamma} = \sin u_{-\gamma}$$

$$\cos\,(u + 4\,m\rfloor\gamma + 4\,\mu\rfloor\beta\, i)_{-\gamma} = \cos u_{-\gamma}$$

$$\Delta\,(u + 2\,m\rfloor\gamma + 4\,\mu\rfloor\beta\, i)_{-\gamma} = \Delta\, u_{-\gamma} \tag{20}$$

The general problem of transformation may be stated thus: Assuming

$$\int_0^{\varphi} \frac{d\varphi}{\sqrt{a^2 - c^2 \sin^2\varphi}} = \int_0^{\varphi'} \frac{d\varphi'}{\sqrt{a'^2 - c'^2 \sin^2\varphi'}} \text{ or } \frac{1}{a}\,\varphi_\gamma = \frac{1}{a'}\,\varphi'_{\gamma'} \tag{21}$$

then it is required to discover the relations between the given quantities φ, a, γ and φ', a', γ'.

Before treating of the special subject of this paper (the quadric transformation), a short exposition of some important points of the general problem of transformation, slightly modified from Abel (see Enneper's Elliptische Functionen, page 239–246), will be given.

We have, by (21),

$$\sin\varphi = \sin\left(\frac{a}{a'}\,\varphi'_{\gamma'}\right)_{-\gamma} = f(\sin\varphi') = f\left\{\sin\left(\frac{a'}{a}\,\varphi_\gamma\right)_{-\gamma'}\right\} \tag{22}$$

where f denotes the unknown relation between $\sin\varphi$ and $\sin\varphi'$.

But we have, by (20),

$$\sin\,(\varphi'\gamma' + 4\,m'\,\rfloor\gamma' + 2\,\mu'\,\rfloor\beta'\,i)\text{-}\gamma'$$

$$= \sin\left\{\frac{a'}{a}\,(\varphi\gamma + 4\,m\,\rfloor\gamma + 2\,\mu\,\rfloor\beta\,i)\right\}\text{-}\gamma' \qquad (23)$$

therefore,

$$m'\,\rfloor\gamma' = \frac{a'}{a}\,m\,\rfloor\gamma \qquad (24)$$

$$\mu'\,\rfloor\beta' = \frac{a'}{a}\,\mu\,\rfloor\beta \qquad (25)$$

$$\therefore \frac{a'}{a} = \frac{m'}{m}\cdot\frac{\rfloor\gamma'}{\rfloor\gamma} = \frac{\mu'}{\mu}\cdot\frac{\rfloor\beta'}{\rfloor\beta}$$

and

$$\frac{m'}{\mu'}\cdot\frac{\rfloor\gamma'}{\rfloor\beta'} = \frac{m}{\mu}\cdot\frac{\rfloor\gamma}{\rfloor\beta}$$

Anticipating here the definition of the highly important constant, the nome q, which is such a prominent feature in the brilliant researches of Jacobi and Abel, we have

$$q = e^{-2\frac{\rfloor\beta}{\rfloor\gamma}\rfloor} \qquad (26)$$

and the nome q' of the transformed integral is

$$q' = e^{-2\frac{\rfloor\beta'}{\rfloor\gamma'}\rfloor} = e^{-2\frac{m'\,\mu\,\rfloor\beta}{\mu'\,m\,\rfloor\gamma}\rfloor} = q^{\frac{m'\,\mu}{\mu'\,m}} \qquad (27)$$

Thus it appears that the nomes of the given and transformed integrals are in a relation

$$q^n = q'^{n'}$$

where n and n' are integers, and, if $n = 1$ and $n' = 2$, we have the quadric transformation.

Landen's transformation consists in assuming

$$\sin\,(2\varphi' - \varphi) = \frac{c}{a}\sin\varphi \qquad (28)$$

which is Legendre's convenient form for computing the amplitude φ'. Differentiating, we have

$$(2d\varphi' - d\varphi)\cos\,(2\varphi' - \varphi) = \frac{c}{a}\cos\varphi\,d\varphi$$

or

$$\frac{d\varphi}{a\,\Delta\,\varphi} = \frac{d\varphi'}{\frac{1}{2}\,(a\,\Delta\,\varphi + c\cos\varphi)} \qquad (29)$$

Solving for φ, we have

$$\tan\varphi = \frac{a \sin 2\varphi'}{c + a\cos 2\varphi'} = \frac{a \tan\varphi'}{a' - b' \tan^2\varphi'} \tag{30}$$

$$\sin\varphi = \frac{2a \sin\varphi' \cos\varphi'}{\sqrt{a^2 + 2ac\cos 2\varphi' + c^2}} = \frac{a \sin\varphi' \cos\varphi'}{a' \Delta\varphi'} \tag{31}$$

$$\cos\varphi = \frac{c + a\cos 2\varphi'}{2a' \Delta\varphi'} = \frac{1}{c}\left(a' \Delta\varphi' - \frac{b'}{\Delta\varphi'}\right) \tag{32}$$

$$\Delta\varphi = \frac{1}{a}\left(a' \Delta\varphi' + \frac{b'}{\Delta\varphi'}\right) \tag{33}$$

where we have placed

$$\tfrac{1}{2}(a + c) = a'\,;\ \tfrac{1}{2}(a - c) = b'\,;$$

$$\sqrt{ac} = c'\,;\ \sqrt{a'^2 - c'^2 \sin^2\varphi'} = a' \Delta\varphi' \tag{34}$$

From (32) and (33) follows

$$a' \Delta\varphi' = \tfrac{1}{2}(a \Delta\varphi + c\cos\varphi) \tag{35}$$

$$\frac{b'}{\Delta\varphi'} = \tfrac{1}{2}(a \Delta\varphi - c\cos\varphi) \tag{36}$$

and (29) becomes
$$\frac{d\varphi}{a \Delta\varphi} = \frac{d\varphi'}{a' \Delta\varphi'} \tag{37}$$

the integral is
$$\frac{1}{a}\varphi\gamma = \frac{1}{a'}\varphi'\gamma' \tag{38}$$

The first and third formula of (34) give the first step in the algorithm of the arithmetico-geometric mean, and the first two follow from (35) and (36) by placing $\varphi = 0 = \varphi'$, *i. e.*, they are relations at the lower limit of the integrals, corresponding to (35) and (36).

Assuming
$$\sin(2\varphi'' - \varphi') = \frac{c'}{a'}\sin\varphi' \tag{28'}$$

$$a'' = \tfrac{1}{2}(a' + c')\,;\ b'' = \tfrac{1}{2}(a' - c')\,;\ c'' = \sqrt{a'c'} \tag{34'}$$

then we have
$$\frac{1}{a}\varphi\gamma = \frac{1}{a'}\varphi'\gamma' = \frac{1}{a''}\varphi''\gamma'' \tag{38'}$$

Proceeding in this manner the amplitudes will very rapidly reach a limit $\varphi^{(\infty)}$, while simultaneously a and c tend to become equal to their common limit, the arithmetico-geometric mean of a and c. Gauss, when investigating its functional properties, denotes this by $M(a, c)$; elsewhere he uses the notation $a^{(\infty)}$ or $c^{(\infty)}$, which is sufficiently distinct for our purpose.

At the limit we have $a^{(\infty)} \Delta \varphi^{(\infty)} = c^{(\infty)} \cos \varphi^{(\infty)}$, therefore,

$$\frac{1}{a}\varphi\gamma = \frac{1}{a'}\varphi'\gamma' = \frac{1}{a''}\varphi''\gamma'' = \ldots\ldots \frac{1}{a^{(\infty)}}\varphi^{(\infty)}$$

$$\left(= \int_0^{\varphi^{(\infty)}} \frac{d\varphi^{(\infty)}}{c^{(\infty)} \cos \varphi^{(\infty)}} = \frac{1}{c^{(\infty)}} \tan \tfrac{1}{2}\left(\lrcorner + \varphi^{(\infty)}\right)\right) \qquad (38^{(\infty)})$$

Let $\varphi = \lrcorner$ then $\varphi' = \lrcorner'$; $\varphi'' = \lrcorner'' \ldots\ldots \varphi^{(\infty)} = \lrcorner^{(\infty)}$ and

$$\frac{1}{a}\lrcorner\gamma = \frac{1}{a'}\lrcorner'\gamma' = \frac{1}{a''}\lrcorner''\gamma'' = \ldots\ldots \frac{1}{a^{(\infty)}}\lrcorner_1^{(\infty)}$$

$$\left(= \frac{1}{c^{(\infty)}} \tan \tfrac{1}{2}\left(\lrcorner + \lrcorner^{(\infty)}\right)\right) \qquad (39^{(\infty)})$$

This transformation can be applied also to the more general form:

$$I = \int_0^{\varphi} \frac{d\varphi}{a\,\Delta\,\varphi} f(\sin \varphi, \cos \varphi, \Delta\,\varphi) \qquad (40)$$

for if, simultaneously to the above algorithm, we express $\sin \varphi$, $\cos \varphi$, $\Delta\,\varphi$ in terms of $\sin \varphi'$, $\cos \varphi'$, $\Delta\,\varphi'$, and these again in terms of $\sin \varphi''$, $\cos \varphi''$, $\Delta\,\varphi''$, etc., by means of (31), (32), (33), we arrive, after a few transformations, at the form

$$I = \int_0^{\varphi^{(\infty)}} \frac{d\varphi^{(\infty)}}{c^{(\infty)} \cos \varphi^{(\infty)}} f^{(\infty)}(\sin \varphi^{(\infty)}, \cos \varphi^{(\infty)}) \qquad (41)$$

which is an elementary form if $f(\sin \varphi, \cos \varphi, \Delta\,\varphi)$ is rational with respect to $\sin \varphi$, $\cos \varphi$, $\Delta\,\varphi$.

In tracing this process backwards, the quantities may be distinguished at the several steps by subprimes, so that we have, at the first backward step,

$$\sin(2\varphi - \varphi_{,}) = \frac{c_{,}}{a_{,}} \sin \varphi_{,} = \frac{a - b}{a + b} \sin \varphi_{,} \qquad (28_{,})$$

$$a = \tfrac{1}{2}(a_{,} + c_{,});\ b = \tfrac{1}{2}(a_{,} - c_{,});\ c = \sqrt{a_{,} c_{,}} \qquad (34_{,})$$

Adding, and then also subtracting, $\sin \varphi_{,}$ from $(28_{,})$ and dividing the difference by the sum, we have the following convenient formula, also given by Legendre:

$$\tan(\varphi_{,} - \varphi) = \frac{b}{a} \tan \varphi \qquad (42)$$

Solving ($34_{,}$) for $a_{,}$, $b_{,}$, $c_{,}$, we have

$$a_{,} = a + b;\ b_{,} = 2\sqrt{ab};\ c_{,} = a - b$$

In order to have again the convenient algorithm of the arithmetico-geometric mean, it is preferable to assume

$$a_1 = \tfrac{1}{2} a_{,} = \tfrac{1}{2}(a + b);\ b_1 = \tfrac{1}{2} b_{,} = \sqrt{ab};\ c_1 = \tfrac{1}{2} c_{,} = \tfrac{1}{2}(a - b) \quad (43)$$

For the second step assume

$$\tan(\varphi_{,,} - \varphi_{,}) = \frac{b_1}{a_1} \tan \varphi_{,} \qquad (42_1)$$

$$a_2 = \tfrac{1}{2}(a_1 + b_1);\ b_2 = \sqrt{a_1 b_1};\ c_2 = \tfrac{1}{2}(a_1 - b_1) \qquad (43_1)$$

We have then $\frac{1}{a}\varphi\gamma = \frac{1}{2a_1}(\varphi_{,})\gamma_1 = \frac{1}{2^2 a_2}(\varphi_{,,})\gamma_2$ $\qquad (44_1)$

Continuing this process, which diminishes the modulus, and is therefore called descending the scale of moduli, while the above is called ascending, the a and b will rapidly approach their arithmetico-geometric mean, $a_\infty = b_\infty$, while $\frac{1}{2^n}\varphi_{(n)}$ tends towards a limit which I shall denote ψ_∞. The limiting form of the integral is

$$\int_0^{\varphi(\infty)} \frac{d\varphi_{(\infty)}}{2^\infty b_\infty} = \frac{\psi_\infty}{b_\infty}$$

and we have

$$\frac{1}{a}\varphi\gamma = \frac{1}{2a_1}(\varphi_{,})\gamma_1 = \frac{1}{2^2 a_2}(\varphi_{,,})\gamma_2 = \dots\ \frac{1}{2^\infty a_\infty}(\varphi_\infty)_0 \left(= \frac{\psi_\infty}{b_\infty}\right. \quad (44^{(\infty)})$$

If $\varphi = \lrcorner$ then $\tfrac{1}{2}\varphi_{,} = \frac{1}{2^2}\varphi_{,,} = \dots \frac{1}{2^n}\varphi_{(n)} = \dots = \lrcorner$

and we have

$$\frac{1}{a}\lrcorner\gamma = \frac{1}{a_1}\lrcorner\gamma_1 = \frac{1}{a_2}\lrcorner\gamma_2 = \dots\dots\ \frac{1}{a_\infty}\lrcorner_0 \left(= \frac{1}{b_\infty}\lrcorner\right. \quad (45_{(\infty)})$$

This remarkable value for the complete integral was discovered by Gauss by means of a different transformation, known as Gauss'. This may be deduced as follows: Assume in place of ($44^{(\infty)}$) the following series of relations

$$\frac{1}{a}\varphi\gamma = \frac{1}{a_1}(\psi'_1)\gamma_1 = \frac{1}{a_2}(\psi'_2)\gamma_2 = \dots\ \frac{1}{a_\infty}(\psi_\infty)_0 \left(= \frac{1}{b_\infty}\psi_\infty\right. \quad (44_\infty)$$

To discover the relations for the first step we have to determine ψ_1 from the equations

$$(\psi_1)\gamma_1 = \tfrac{1}{2}(\varphi_{,})\gamma_1 = \frac{a_1}{a}\varphi\gamma \qquad (46)$$

Place in (12) $u = (\varphi_{,})\gamma_1$, then $\frac{1}{2}u = (\psi_1)\gamma_1$ and $u_{-\gamma_1} = \varphi_{,}$; $\left(\frac{u}{2}\right)_{-\gamma_1} = \psi_1$, and consequently

$$\begin{aligned} \sin^2\psi_1 &= 1 - \cos\varphi_{,} && \div 1 + \Delta\varphi_{,} \\ \cos^2\psi_1 &= \Delta\varphi_{,} + \cos\varphi_{,} && \div 1 + \Delta\varphi_{,} \\ \Delta^2\psi_1 &= \beta_1^2 + \Delta\varphi_{,} + \gamma_1^2\cos\varphi_{,} && \div 1 + \Delta\varphi_{,} \end{aligned} \qquad (47)$$

From (32) and (33) we derive with due regard to (43)

$$\begin{aligned} a_1\,\Delta\varphi_{,} &= \tfrac{1}{2}\left(a\,\Delta\varphi + \frac{b}{\Delta\varphi}\right) \\ c_1\,\Delta\varphi_{,} &= \tfrac{1}{2}\left(a\,\Delta\varphi - \frac{b}{\Delta\varphi}\right) \end{aligned} \qquad (48)$$

and eliminating $\varphi_{,}$ from (47) by means of (48) there result the relations

$$\begin{aligned} \sin^2\psi_1 &= \frac{a_1}{c_1}\cdot\frac{1-\Delta\varphi}{1+\Delta\varphi} \\ \cos^2\psi_1 &= \frac{a\,\Delta\varphi - b}{c_1(1+\Delta\varphi)} \\ \Delta^2\psi_1 &= \frac{a\,\Delta\varphi + b}{a_1(1+\Delta\varphi)} \end{aligned} \qquad (49)$$

whence also

$$\begin{aligned} \sin\varphi &= \frac{a\sin\psi_1}{a_1 + c_1\sin^2\psi_1} \\ \cos\varphi &= \frac{a_1\cos\psi_1\,\Delta\psi_1}{a_1 + c_1\sin^2\psi_1} \\ \Delta\varphi &= \frac{a_1 - c_1\sin^2\psi_1}{a_1 + c_1\sin^2\psi_1} \end{aligned} \qquad (50)$$

This is Gauss' transformation. For practical use it is far less convenient than that given above.

Instead of (46) we might have assumed

$$m(\theta_1)\gamma_1 = n(\varphi_{,})\gamma_1 = 2n\frac{a_1}{a}\varphi\gamma \quad (m \text{ and } n \text{ integers}) \qquad (51)$$

For any special values of m and n we can express, by means of the addition theorem, the elliptic functions of $\left\{m\,(\theta_1)\gamma_1\right\}_{-\gamma_1}$ in terms of those of θ_1, and in the same manner those of $\left\{n\,(\varphi_{,})\gamma_1\right\}_{-\gamma_1}$ in terms of those of $\varphi_{,}$. Since we know φ in terms of $\varphi_{,}$, we can eliminate $\varphi_{,}$ and obtain a relation between θ_1 and φ, which would be a new transformation. However, we need not expect to discover in this manner any substitution sufficiently simple for practical use.

The substitutions given above may of course be applied also to the complementary integral, and, since interesting relations will be thus discovered, I place the different series of forms together for comparison.

$$\frac{1}{a}\varphi\gamma = \frac{1}{a'}\varphi'\gamma' = \frac{1}{a''}\varphi''\gamma'' = \ldots\ldots\ldots\ldots = \frac{1}{a^{(\infty)}}\varphi_1^{(\infty)}$$

$$\left(= \frac{1}{c^{(\infty)}}\,l \tan \tfrac{1}{2}\,(\lrcorner + \varphi^{(\infty)})\right)$$

$$= \frac{1}{2a_1}(\varphi_{,})\gamma_1 = \frac{1}{2^2 a_2}(\varphi_{,,})\gamma_2 = \ldots\ldots\ldots = \frac{1}{a_\infty}\lim \frac{\varphi_{(n)}}{2^n}$$

$$\left(= \frac{1}{b_\infty}\psi_\infty\right.$$

$$= \frac{1}{a_1}(\psi_1)\gamma_1 = \frac{1}{a_2}(\psi_2)\gamma_2 = \ldots\ldots\ldots = \frac{1}{a_\infty}(\psi_\infty)_0$$

$$\left(= \frac{1}{b_\infty}\psi_\infty\right. \qquad (52\gamma)$$

$$\frac{1}{a}\varphi\beta = \frac{1}{a_1}(\varphi_1)\beta_1 = \frac{1}{a_2}(\varphi_2)\beta_2 = \ldots\ldots\ldots = \frac{1}{a_\infty}(\varphi_\infty)_1$$

$$\left(= \frac{1}{b_\infty}\,l \tan \tfrac{1}{2}\,(\lrcorner + \varphi_\infty)\right)$$

$$= \frac{1}{2a_1}(\varphi_{-1})\beta' = \frac{1}{2^2 a''}(\varphi_{-2})\beta'' = \ldots\ldots = \frac{1}{a^{(\infty)}}\lim \frac{\varphi_{-n}}{2^n}$$

$$\left(= \frac{1}{c^{(\infty)}}\psi^{(\infty)}\right.$$

$$= \frac{1}{a'}\psi'\beta' = \frac{1}{a''}\psi''\beta'' = \ldots\ldots\ldots\ldots = \frac{1}{a^{(\infty)}}\psi_0^{(\infty)}$$

$$\left(= \frac{1}{c^{(\infty)}}\psi^{(\infty)}\right. \qquad (52\beta)$$

$$\frac{1}{a}\lrcorner\gamma = \frac{1}{a'}\lrcorner'\gamma' = \frac{1}{a''}\lrcorner''\gamma'' = \ldots\ldots\ldots\ldots = \frac{1}{a^{(\infty)}}\lrcorner_1^{(\infty)}$$

$$\left(= \frac{1}{c^{(x)}}\, l \tan \tfrac{1}{2} (\lrcorner + \lrcorner^{(\infty)})\right.$$

$$= \frac{1}{a_1} \lrcorner \gamma_1 = \frac{1}{a_2} \lrcorner \gamma_2 = \ldots\ldots\ldots\ldots = \frac{1}{b_\infty} \lrcorner \qquad (53\gamma)$$

$$\frac{1}{a} \lrcorner \beta = \frac{1}{a_1} (\lrcorner_1) \beta_1 = \frac{1}{a_2} (\lrcorner_2) \beta_2 = \ldots\ldots\ldots = \frac{1}{b_x} (\lrcorner^{(\infty)})_1$$

$$\left(= \frac{1}{b_\infty}\, l \tan \tfrac{1}{2} (\lrcorner + \lrcorner_\infty)\right.$$

$$= \frac{1}{a'} \lrcorner \beta' = \frac{1}{a''} \lrcorner \beta'' = \ldots\ldots\ldots\ldots = \frac{1}{c^{(\infty)}} \lrcorner \qquad (53\beta)$$

We easily deduce the symmetrical relations

$$\varphi_1^{(\infty)} (\varphi_\infty)_1 = \varphi'_\infty \varphi_1'^{(\infty)} \qquad (54)$$

$$\lrcorner_1^{(x)} (\lrcorner_\infty)_1 = \lrcorner^2 \qquad (55)$$

This last equation is well known; it appears here, however, as a particular case of a more general relation. The quantity φ'_∞ is the argument of the θ functions and then usually denoted x; $\varphi_1^{(x)}$ is then denoted by x'; Schellbach has $\frac{\nu}{2}$ for $(\lrcorner_\infty)_1$ and $\frac{\nu'}{2}$ for $\lrcorner_1^{(x)}$, while Hoüel, in his Recueil de Tables, has ρ and ρ', respectively.

Other relations are

$$\frac{\varphi_1^{(x)}}{\lrcorner_1^{(x)}} = \frac{\varphi'_x}{\lrcorner}; \quad \frac{\varphi_1^{(x)}}{\lrcorner} = \frac{\varphi'_x}{(\lrcorner_x)_1}; \quad \frac{\varphi_1^{(x)2}}{\lrcorner_1^{(x)}} = \frac{\varphi_1^{(\infty)} \varphi'_\infty}{\lrcorner} = \frac{\varphi'^2_x}{(\lrcorner_x)_1} \qquad (56)$$

$$\frac{(\varphi_x)_1}{(\lrcorner_x)_1} = \frac{\varphi_1'^{(x)}}{\lrcorner}; \quad \frac{(\varphi_x)_1}{\lrcorner} = \frac{\varphi_1'^{(x)}}{\lrcorner_1^{(x)}}; \quad \frac{(\varphi_x)_1^2}{(\lrcorner_x)_1} = \frac{(\varphi_x)_1 \varphi_1'^{(x)}}{\lrcorner} = \frac{\varphi_1'^{(x)2}}{\lrcorner_1^{(x)}} \qquad (57)$$

The following expressions for the nome q can now be given:

$$q = e^{-2 \frac{\lrcorner \beta}{\lrcorner \gamma}} = e^{-2 (\lrcorner_x)_1}$$

$$= e^{-2 \frac{b_x}{c^{(x)}} \lrcorner} = e^{-2 \frac{b_x}{a} \lrcorner \beta} \qquad (58)$$

The first form is simply Jacobi's definition; the second gives, since

$$(\lrcorner_x)_1 = l \tan \tfrac{1}{2} (\lrcorner + \lrcorner_x) \qquad (59)$$

$$q = \cot^2 \tfrac{1}{2} (\lrcorner + \lrcorner_x) \qquad (60)$$

This is one of the best formulæ for computing q, especially if the modulus does not differ much from unity. The third form may be

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used if b and c are not very different, for in that case the algorithm of the arithmetico-geometric mean converges equally fast in both directions. If either b or c is very near to a, the process may converge in one direction so slowly that the formula becomes nearly inapplicable.

The fourth form may be transformed to a new formula, which is more convenient than any given. In (52β) place $\varphi = 2^n \lrcorner$, then, since

$$\varphi_1 = 2^{n-1} \lrcorner\,;\ \varphi_1 = 2^{n-2} \lrcorner\,;\ \varphi_3 = 2^{n-3} \lrcorner \ldots .$$
$$\varphi_n = \lrcorner\,;\ \varphi_{n+1} = (2^n \lrcorner)_{n+1} \ldots .$$

we have

$$\frac{2^n}{a} \lrcorner\beta = \frac{2^{n-1}}{a_2} \lrcorner\beta_1 = \frac{2^{n-2}}{a_2} \lrcorner\beta_2 = \ldots .$$
$$= \frac{1}{a_n} \lrcorner\beta_n = \frac{1}{a_{n+1}} \left\{ (2^n \lrcorner)_{n+1} \right\} \beta_{n+1} \qquad (61)$$

But we have by (28) $\quad \sin\left(2(2_n \lrcorner)_{n+1} - \lrcorner\right) = \frac{b_n}{a_n} \sin \lrcorner$

or $\quad 2 \sin^2 (2^n \lrcorner)_{n+1} - 1 = \frac{b_n}{a_n}$

$$\therefore \sin (2^n \lrcorner)_{n+1} = \sqrt{\tfrac{1}{2}\left(1 + \frac{b_n}{a_n}\right)}$$

If we suppose $a_n = b_n = b_\infty$ within the precision of the computation, c_n will be very small, yet not zero. We have then

$$\frac{1}{a^\infty} \lrcorner\, \beta_n = \frac{1}{a_{n+1}} \left\{(2^n \lrcorner)_{n+1}\right\} \beta_{n+1}$$
$$= \frac{1}{a_{n+1}}\, l \tan \tfrac{1}{2} \left(\lrcorner + (2^n \lrcorner)_{n+1}\right)$$
$$= \frac{1}{a_{n+1}}\, l \sqrt{\frac{1 + \sin (2_n \lrcorner)_{n+1}}{1 - \sin (2^n \lrcorner)_{n+1}}}$$
$$= \frac{1}{a_{n+1}}\, l \frac{\sqrt{1 + \sqrt{\tfrac{1}{2}\left(1 + \frac{b_n}{a_n}\right)}}}{\sqrt{1 + \sqrt{\tfrac{1}{2}\left(1 + \frac{b_n}{a_n}\right)}}}$$

$$= \frac{1}{a_{n+1}} \, l \, \frac{1 + \sqrt{\frac{1}{2}\left(1 + \frac{b_n}{a_n}\right)}}{\sqrt{\frac{1}{2}\left((1 - \frac{b_n}{a_n}\right)}}$$

$$= \frac{1}{b_\infty} \, l \, \frac{2\sqrt{a_n}}{\sqrt{\frac{1}{2}(a_n - b_n)}} \quad \text{(sufficiently near)}$$

$$= \frac{1}{b_\infty} \, l \, \frac{2^2\sqrt{a_n a_{n+1}}}{c_n} \quad \text{(sufficiently near)}$$

$$= \frac{1}{b_\infty} \, l \, \frac{2^2 a_n}{c_n} \quad \text{(sufficiently near)} \qquad (62)$$

therefore we have by (61)

$$\frac{1}{a} \lrcorner \beta = \frac{2^{-n}}{b_\infty} \, l \, \frac{2^2 a_n}{c_n}$$

or $$\frac{b_\infty}{a} \lrcorner \beta = l \left(\frac{2^2 a_n}{c_n}\right)^{2^{-n}}$$

$$l \left(2^2 a_n \cdot \frac{2^2 a_n}{c^2_{n-1}}\right)^{2^{-n}} = l \left(\frac{2^2 a_n}{c_{n-1}}\right)^{2^{-n+1}}$$

$$\text{since } c_n = \tfrac{1}{2}(a_{n-1} - b_{n-1}) = \frac{c^2_{n-1}}{2^2 a_n}$$

$$= l \left(2^2 a_n \cdot \frac{2^2 a_{n-1}}{c^2_{n-2}}\right)^{2^{-n+1}} = l \left(\frac{2^2 a_{n-1}}{c_{n-2}}\right)^{2^{-n+2}}$$

$$\text{if } a_{n-1} = \sqrt{a_n a_{n-1}}$$

$$= l \left(2^2 a_{n-1} \cdot \frac{2^2 a_{n-2}}{c^2_{n-3}}\right)^{2^{-n+2}} = l \left(\frac{2_n a_{n-2}}{c_{n-3}}\right)^{2^{-n+3}}$$

$$\text{if } a_{n-2} = \sqrt{a_{n-1} a_{n-2}}$$

. . . .

$$= l \left(2^2 a_3 \cdot \frac{2^2 a_2}{c_1^2}\right)^{2^{-2}} = l \left(\frac{2^2 a_2}{c_1}\right)^{2^{-1}}$$

$$\text{if } a_2 = \sqrt{a_3 a_2} \qquad (63)$$

$$= l \left(2^2 a_2 \cdot \frac{2^2 a_1}{c^2}\right)^{2^{-1}} = l \left(\frac{2^2 a_1}{c}\right)$$

$$\text{if } a_1 = \sqrt{a_2 a_1} \qquad (64)$$

Using (63) in the fourth form of (58) we have

$$q = \frac{c_1}{2^2 a_2} = \frac{a-b}{2^3 a_2} \tag{65}$$

and using (64) we have

$$q = \left(\frac{c}{2^2 a_1}\right)^2 \tag{66}$$

The nome of the complementary integral is denoted by Jacobi and writers that follow him by q'. In our system this would be the notation for the nome of the integral $\varphi'\gamma'$; q'' that for $\varphi''\gamma''$, etc; also q_1 that of $(\varphi'_1)\gamma_1$; q_2 of $(\varphi'_2)\gamma_2$, etc. It is therefore better to follow the example of Broch, who denotes the nome of the complementary integral by p. We have then

$$p = e^{-2\frac{\rfloor\gamma}{\rfloor\beta}\rfloor} = e^{-2\rfloor_1^{(x)}} = e^{-2\frac{c^{(x)}}{b_x}\rfloor}$$

$$= \cot^2 \tfrac{1}{2}(\rfloor + \rfloor^{(x)}) = \frac{a-c}{2^3 a''} = \left(\frac{b}{2^2 a'}\right)^2 \tag{67}$$

where

$$\begin{aligned} a^{(n-1)} &= \sqrt{a^{(n)}\, a^{(n-1)}} \\ a^{(n-2)} &= \sqrt{a^{n-1}\, a^{n-2}} \\ a^{(n-3)} &= \sqrt{a^{n-2}\, a^{n-3}} \\ &\cdots\cdots\cdots \\ a'' &= \sqrt{a'''\, a''} \\ a' &= \sqrt{a''\, a'} \end{aligned} \tag{68}$$

By (55) and the second forms of (58) and (67) we have the following relation between p and q

$$l\,p^{-\frac{1}{2}}\, l\,q^{-\frac{1}{2}} = \rfloor^2 \tag{69}$$

or in Briggian logarithms

$$\log\left\{\log p^{-\frac{1}{2}} \log q^{-\frac{1}{2}}\right\} = \log(\rfloor \log e)^2 = 9.6678084 \tag{69'}$$

or $$\log\left\{\log p^{-1}\ \log q^{-1}\right\} = 0.2698684 \tag{70}$$

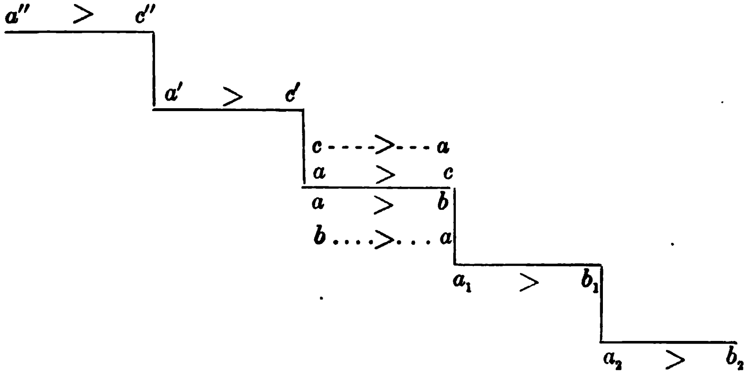
By means of this relation we can always choose the shortest route to either p or q. It is easy to see that the nomes and complementary nomes at the several steps of the modular scale are as follows

$$q_n = q^{2^{-n}} \ldots . q_2 = q^{2^{-2}}\ ;\ q_1 = q^{2^{-1}}\ q = q;\ q' = q^2;\ q''$$
$$= q^{2^2} \ldots .. q^{(n)} = q^{2^n} \qquad (71)$$

$$p_n = p^{2^n} \ldots . p_2 = p^{2^2}\ ;\ p_1 = p^2;\ p = p;\ p' = p^2;\ p''$$
$$= p^{2^{-2}} \ldots .. p^{(n)} = p^{2^{-n}} \qquad (72)$$

We have then in this transformation the simplest possible case of Abel's theorem (27); and because in ascending we pass to the square of the nome, it is called the quadric transformation.

The ascending transformation is possible in real quantities if $c > a$, for we have $M(c, a) = M(a, c)$. Also if $b > a$ we can use the descending transformation; and in either case we can, after one transformation, proceed in either direction. This may be symbolized by the following diagram



In order to exhibit the practical nature of the formulæ given, I shall make the necessary computations for the integral

$$u = \int_0^{\varphi} \frac{d\varphi}{\sqrt{1 - \sin^2 75 \sin^2 \varphi}}$$

if $\varphi = 70^\circ$ and also for the complete integral.

Because $\gamma = \sin 75^\circ$ is $> \sqrt{\tfrac{1}{2}}$ we must use the ascending transformation. The computation for $70^\circ_{\sin 75^\circ}$ may be conveniently arranged as follows:

$a = 1$	$\log c = 9.9849438$	$\varphi = 70^{\circ}00'00.''00$	$\log \sin \varphi = 9.9729858$
$c = 0.9659258$	$\log a = 0.0000000$	$2\varphi' - \varphi = 65\ 11\ 08.\ 84$	$\log \sin (2\varphi' - \varphi) = 9.9579296$
$c' = 0.9829629$	$\log c' = 9.9924719$	$\varphi' = 67\ 35\ 34.\ 42$	$\log \sin \varphi' = 9.9659063$
	$\log a' = 9.9925371$	$2\varphi'' - \varphi' = 67\ 34\ 19.\ 33$	$\log \sin (2\varphi'' - \varphi') = 9.9658411$
	$\log c^{(\infty)} = \log c'' = 9.9925045$	$\varphi^{(\infty)} = \varphi'' = 67\ 34\ 56.\ 88$	
		$\frac{1}{2}(\lrcorner + \varphi^{(\infty)}) = 78\ 47\ 28.\ 44$	$\log \tan \frac{1}{2}(\lrcorner + \varphi^{(\infty)}) = 0.7029746$
			$\log \log \tan \frac{1}{2}(\lrcorner + \varphi^{(\infty)}) = 9.8469397$
			$- \log \log e = -\ 9.6377843$
			$- \log c^{(\infty)} = -\ 9.9925045$
			$\log 70^{\circ}_{\sin 75^{\circ}} = 0.2166509$

The computation of $\lrcorner_{\sin 75^{\circ}}$ by this process requires exactly the same amount of computation.

(By 53γ) and (67), we have

$$\frac{1}{a} \lrcorner \gamma = \frac{1}{c^{(\infty)}} \lrcorner_1^{(\infty)} = \frac{1}{c^{(\infty)}}\, l\, p^{-\frac{1}{2}} = \frac{1}{c^{(\infty)}}\, l \sqrt{\frac{2^3 a''}{a - c}} = \frac{1}{c^{(\infty)}}\, l\, \frac{2^2 a'}{b} \qquad (73)$$

We compute then the complementary nome and from that the complete integral is found easily.

$$
\begin{array}{lrcl}
 & -\log a'' & = & -\,9.9925045 \\
 & -\log 2^{s} & = & -\,0.9030900 \\
a - c = 0.0340742 & \log(a-c) & = & 8.5324257 \\
\hline
 & \log p & = & 7.6368312 \\
\text{Computation of } \rfloor_{\sin 75^\circ} & \log p^{-\frac{1}{2}} & = & 1.1815844 \\
\hline
 & \log\log p^{-\frac{1}{2}} & = & 0.0724648 \\
 & -\log\log e & = & -\,9.6377843 \\
 & -\log c^{(\infty)} & = & -\,9.9925045 \\
\hline
 & \log \rfloor_{\sin 75^\circ} & = & 0.4421760
\end{array}
$$

This method is far more simple than by amplitudes.

The computation of the same integral by the descending scale will now be shown, though it is longer in this case, and therefore less accurate.

$a = 1$ $b = 0.2588190$	$\log b = 9.4129962$ $\log a = 0.0000000$	$\varphi = 70°00'00.''00$ $\varphi_{,} - \varphi = 35\ 24\ 59.\ 80$	$\log \tan \varphi = 0.4389341$ $\log \tan (\varphi_{,} - \varphi) = 9.8519303$
$a_1 = 0.6294095$ $b_1 = 0.5087425$	$\log b_1 = 9.7064981$ $\log a_1 = 9.7989333$	$\varphi_{,} = 105\ 24\ 59.\ 80$ $\varphi_{,,} - \varphi_{,} = 108\ 50\ 16.\ 04$	$\log \tan \varphi_{,} = 0.5594721_n$ $\log \tan (\varphi_{,,} - \varphi_{,}) = 0.4670369_n$
$a_2 = 0.5690760$ $b_2 = 0.5658687$	$\log b_2 = 9.7527157$ $\log a_2 = 9.7551704$	$\varphi_{,,} = 214\ 15\ 15.\ 84$ $\varphi_{,,,} - \varphi_{,,} = 214\ 06\ 14.\ 04$	$\log \tan \varphi_{,,} = 9.8331396$ $\log \tan (\varphi_{,,,} - \varphi_{,,}) = 9.8306850$
$a_3 = 0.5674724$	$\log b_3 = 9.7539430$ $\log a_3 = 9.7539447$	$\varphi_{,,,} = 428\ 21\ 29.\ 88$ $\varphi_{\text{iv}} - \varphi_{,,,} = 428\ 21\ 29.\ 60$	$\log \tan \varphi_{,,,} = 0.4014610$ $\log \tan (\varphi_{\text{iv}} - \varphi_{,,,}) = 0.4014593$
	$\log b_\infty = \log b_4 = 9.7539439$	$\varphi(\infty) = \varphi_{\text{iv}} = 856\ 42\ 59.\ 48$ $\psi_\infty = 53\ 32\ 41.\ 22$ $= 192761.22$	

$$\begin{aligned}
\log \psi_\infty &= 5.2850197 \\
\log \text{arc } 1'' &= 4.6855749 \\
-\log b_\infty &= -9.7539439
\end{aligned}$$

Computation of $\rfloor_{\sin 75°}$

$$\begin{aligned}
\log 70°_{\sin 75°} &= -0.2166507 \\
\log \rfloor &= 0.1961199 \\
-\log b_\infty &= -9.7539439 \\
\log \rfloor_{\sin 75°} &= 0.4421760
\end{aligned}$$

The computation of the nome q stands thus:

$$\begin{array}{lrcl} & \log a_3 & = & 9.7539441 \\ \hline & -\log a_2 & = & -\ 9.7545572 \\ & -\log 2^3 & = & -\ 0.9030900 \\ a - b = 0.7411810 & \log a - b & = & 9.8699243 \\ \hline & \log q & = & 9.2122771 \end{array}$$

To check by (70), we have

$$\begin{array}{ll} \log p^{-1} = 2.3631688 & \log \log p^{-1} = 0.3734948 \\ \log q^{-1} = 0.7877229 & \log \log q^{-1} = 9.8963735 \\ \hline & 0.2698683 \end{array}$$

The above incomplete sketch is, I hope, sufficient to show the practical advantages from the introduction of the algorithm of the arithmetico-geometric mean into the theory of elliptic functions.

Mr. HALL spoke of the importance of the arithmetico-geometric mean in astronomy.

Mr. W. B. TAYLOR made a communication on

A CASE OF DISCONTINUITY IN ELLIPTIC ORBITS

around an empty center of gravitative force. Diminution of the minor axis of the attracted body's path (the major axis being constant) increases the ratio of distance at the two apses without limit, the "periapsis" continually approaching the attractive center, as long as the minor axis has a value, however small. But when this axis is made to vanish, and the motion is directly to the center of force. the body, instead of rebounding from it, as continuity would require, will pass through it, and describe an equal path on the opposite side, the orbit being at once doubled.

This paper was discussed by Messrs. BATES, CHRISTIE, HALL and others, and brought out a wide diversity of view as to the demeanor of a heavy point when coincident with an empty attracting center.

15TH MEETING. DECEMBER 3, 1884.

The Chairman presided.

Nineteen members and guests present.

Mr. M. H. DOOLITTLE made a communication on

THE VERIFICATION OF PREDICTIONS.

[Abstract.]

Mr. G. K. Gilbert has published (American Meteorological Journal, 8°, *Detroit;* September, 1884, pp. 166–172) a method of estimating the ratio of skill in predictions of occurrences and non-occurrences of a simple event. Adopting his notation, we have

$s =$ the sum or total number of cases,

$o =$ the number of occurrences,

$p =$ the number of predictions of occurrences,

c = the number of coincidences or verifications,

i = the inference-ratio, or that part of the success which is due to skill and not to chance, and which may be called the *degree of logical connection* between event and prediction.

Since success is proportional to each of the two fractions

$$\frac{c}{o} \text{ and } \frac{c}{p},$$

it may be represented by their product

$$\frac{c^2}{op}.$$

The fraction $\frac{o}{s}$ represents the ratio of random success, and therefore $\frac{op}{s}$ verifications out of p predictions are to be ascribed to chance and must be subtracted throughout. The remainders,

$$o - \frac{op}{s} \text{ and } p - \frac{op}{s},$$

represent fields which chance leaves for science to conquer; and

$$c - \frac{op}{s}$$

represents the portion of each which science does conquer. Hence

$$i = \frac{c - \frac{op}{s}}{o - \frac{op}{s}} \times \frac{c - \frac{op}{s}}{p - \frac{op}{s}} = \frac{(cs - op)^2}{op(s - o)(s - p)}.$$

By another method,

$\frac{c}{o}$ = the probability that any single occurrence will be predicted in some manner.

$\frac{p - c}{s - o}$ = the probability that any single date of non-occurrence will correspond to an unsuccessful prediction = the general probability of unskillful prediction in any case.

Subtract from the probability that any single occurrence will be predicted in some manner the general probability of unskillful prediction, and we have

$\frac{c}{o} - \frac{p - c}{s - o}$ = the probability that any given occurrence will be skillfully predicted.

slowly with that of s, diminishes with increase of o or p, and varies between the limits 0 and 1. Skill in making false predictions is indicated by a negative value of $cs - op$; but the same degree of causal relation exists as when equal skill is employed in making true predictions; and a negative value of i can never occur. When $s =$ either p or o, $i = \frac{0}{0}$; but the apparent indeterminateness vanishes when we consider that i is the product of two factors, of which one $= 0$ and the other is indeterminate within limits. And the value of i is unaltered when predictions of non-occurrences are substituted for those of occurrences, and *vice versa*. In the latter case, write $s - o$ for o, $s - p$ for p, and $s - o - p + c$ for c; and the formula reduces to its original form.

In addition to Mr. Gilbert's tests, two others may be considered. In the case of predictions all falsely reported, we may write $s - p$ for p and $o - c$ for c; and the formula becomes

$$i = \frac{(op - cs)^2}{op\,(s - o)\,(s - p)},$$

with a proper reversal of signs in the quantity under the exponent and no change in the value of i.

If occurrences always appear whenever they are not predicted, and never appear when they are predicted, we put $c = 0$ and $p = s - o$, with the result

$$i = 1;$$

or the logical connection is perfect.

In order that the general formula shall be properly applicable, care must be taken that the predictions are fairly homogeneous in definiteness of time and space. For illustration: if predictions that phenomena will occur in given months are examined indiscriminately with those that they will occur on given days, the result will be manifestly worthless.

It has been proposed to extend the problem so as to include three or more classes of events of which one must happen and only one can happen in any case. It seems clear to me that no single numerical expression can be a proper solution of such a problem. Suppose the three classes of events, A, B, and C. By the method above given A and Not A may be examined; and all instances involving either the prediction or occurrence of A may be excluded and B and C separately investigated. Suppose it thus ascertained that great skill has been shown in discriminating between A and Not A, and little or none in discriminating between B and C. No single numerical expression can properly comprehend these heterogeneous results.

Mr. CURTIS showed that some of the results given by Mr. Doolittle could be independently deduced by another method.

Mr. GILBERT noted as a defect in the formula proposed by Prof. Peirce, that it did not duly discourage positive predictions of rare events; and, while gratified with Mr. Doolittle's discussion of the subject, he expressed a disappointment that no satisfactory decision as to the treatment of cases of three or more alternatives had been reached by him.

After some further discussion, a communication by Mr. M. BAKER was called, but postponed, on motion of Mr. H. FARQUHAR, to allow time for the consideration of a testimonial to a late associate, Mr. ALVORD.

Mr. E. B. ELLIOTT read the following tribute, prepared by Mr. BAKER and himself:

MEMORIAL.

The Mathematical Section of the Philosophical Society of Washington, having suffered the loss by death, on October 16th, 1884, of General BENJAMIN ALVORD, one of its founders and active workers, desires to place on record this testimonial to his worth and to the loss to this Section and to science by his death.

Of his worth, one of America's greatest mathematicians has said that he was a scientist of "real originality who had actually extended the boundaries of science."

The bent of General Alvord's mind and studies was early directed towards a purely geometrical solution of the general problem of tangencies, and his reward, which it is our pleasure to chronicle, was success.

Of his mathematical publications, the following is submitted as a provisionally complete list:

LIST OF MATHEMATICAL PUBLICATIONS BY GENERAL BENJAMIN ALVORD.

1. The tangencies of circles and of spheres.
   [*In* Smithsonian Contributions to Knowledge. 4°. Washington, 1856, Vol. 8, Article 4, 16 pp., 9 plates.]
   Also issued separately.

2. On the interpretation of imaginary roots in questions of maxima and minima.
   [*In* The Mathematical Monthly. 4°. New York, 1860, April, Vol. 2, No. 7, pp. 237-240.]

3. Tangencies.
   [*In* Johnson's New Universal Cyclopædia. 8°. New York, 1878, Vol. 4, pp. 723-4.]

4. Mortality in each year among the officers of the army for fifty years, from 1824 to 1873, as derived from the army registers.
   [*In* Proceedings of the American Association for the Advancement of Science, 23d Meeting, Hartford, August, 1874. 8°. Salem, 1875, pp. 57-59.]

5. The intersection of circles and the intersection of spheres.
   [*In* American Journal of Mathematics. 4°. Baltimore, 1882, March, Vol. 5, No. 1, pp. 25-44; 4 plates.]

6. Curious fallacy as to the theory of gravitation.
   [*In* Bulletin of the Philosophical Society of Washington. 8°. Washington, 1883, Vol. 5, pp. 85-88.]

7. A special case in maxima and minima.
   [*In* Bulletin of the Philosophical Society of Washington. 8°. Washington, 1884, Vol. 6, p. 149.]

Mr. M. BAKER, in moving the adoption of this memorial by the Section, said:

General Alvord's entire life was that of the soldier, and his routine of life work did not call him in the direction of mathematical study. Hence whatever he accomplished in mathematics or literature was accomplished in military surroundings and with only such facilities as barrack and camp life afford. If under these conditions the total of his contributions to science appears small, we should bear in mind that any contribution under such circumstances is exceptional. And to have been able, therefore, to make even a single contribution to human knowledge is to have done that which few men in any generation do and that of which any one of us might well be proud.

General Alvord early became interested in the problem of tangencies and intersections of circles, and his chief mathematical work and fame rests on his complete and purely geometrical solution of the various problems relating to this subject. His chief writings on this subject consist of the paper on Tangencies, in the Smithsonian Contributions in 1856; the article on Tangencies, in Johnson's New Universal Cyclopædia; and the paper on intersections, in the American Journal of Mathematics, March, 1882.

The memorial was adopted, and the Secretary was instructed to send a copy of it to the family of the deceased.

NOTE.

The following members have assisted the Chairman and Secretary in the examination of abstracts of communications to the Mathematical Section:

Title.	*Author.*	*Third Member.*
The Problem of the Knight's Tour	G. K. GILBERT.	E. B. ELLIOTT.
Formulæ for Diminution of Amplitude of a Pendulum	H. FARQUHAR.	A. S. CHRISTIE.
The Formulæ for Computing the Position of a Satellite	A. HALL.	C. H. KUMMELL.
The Quadric Transformation of Elliptic Integrals	C. H. KUMMELL.	G. W. HILL.
The Verification of Predictions	M. H. DOOLITTLE.	M. BAKER.

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